Varieties of Unconventional Scientific Realisms

Omar El Mawas

ABSTRACT

Of the many notable scientific realist positions on offer today few provide penetrating insights which promise to transform the very debate. This thesis provides a critical exposition of three positions that I argue do work to transform the debate. I label these 'unconventional' scientific realisms as they go against the more conventional understanding of scientific realism. These are:

- Hilary Putnam's Common Sense Realism
- Nancy Cartwright's Modelling-based Realism
- Hasok Chang's Pragmatic Realism

Common sense realism is the last of Putnam's positions on realism but it is also the least studied and most underappreciated compared to his former positions. It includes a component on direct realism in perception, convergent scientific realism and a form of metaphysical realism. It presents the later Putnam as a metaphysical realist who is keen to reject deflationist accounts of scientific realism and to bring realism back to the common man.

Modelling-based Realism rejects common readings of Cartwright as instrumentalist or as entity realist, by showing that underlying her diverse views on realism is a robust unified position which does not fit within the usual theory-based framework. Modelling-based Realism is a form of model-based particularism that denies that theories and laws express claims and treats them instead as principles that are short-hand labels for powers and our practices for using them. It accepts as representative, when successful, local system-specific models and is committed to theoretical entities which powers are properties of.

Pragmatic Realism, for Chang, aims to reorient the realism debate away from truth and towards practice. It does that by replacing the common proposition-based framework by an action-based alternative. It does away with truth as correspondence for what Chang calls 'operational coherence'. It ultimately accepts a pragmatic theory of truth and reality, whereby a claim is considered true and an entity real if taking them as such is pragmatically necessary to carry out a coherent and successful epistemic activity.

All three positions highlight different places where the scientific realism debate in its current form is defective, such as failing to appreciate the proper place of scientific realism within metaphysical realism thus positing an illusory conflict between science and common sense, committing to theories despite them not being the proper loci of scientific success, and overlooking the crucial role of practice in arguing from success to truth. Each of these three philosophers suggests ways to overcome these different defects. I argue that their three

proposals are all compatible. I conclude by providing some groundwork for a scientific realist position that integrates their diverse insights into a synthetic whole.

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by

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بسم اللہ والحمد للہ والصلاۃ والسلام علی سیدنا رسول اللہ وعلی آلہ وصحبہ ومن والاہ

In the Name of Allah, the Most Gracious, the Most Merciful and Peace and Blessings be Upon his Prophet Muhammad.

If I were to do justice, this page would have been wholly dedicated to Nancy Cartwright, and even then it would still have fallen short. But since I know she would not like that, I will content myself with few comments. Throughout my PhD journey, Nancy has been more than a co-supervisor, more than a mentor and more than a friend. Nancy has been, is, and will continue to be family. She took me under her wing and sowed the seeds of scholarship in me and watched as they began to sprout. She listened to my stupidities with great interest, and corrected me before I said them in public. She paid attention to the tiniest of details (as she painfully does), and taught me to appreciate precision. Credit goes to her for the good philosophy that I am to produce (and I take full responsibility of the bad bits).

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For Hu

Introduction

The scientific realism debate has the capacity to ever renew itself, as a key player in the debate once remarked (Saatsi 2017). With continuous assaults on scientific realism by anti-realists, there has been no shortage of realist responses, and positions in the debate have recently splintered (see Rowbottom 2019). Of the many notable scientific realist positions on offer today few, however, provide penetrating insights which promise to transform the very debate. This thesis provides a critical exposition of three such positions. I label these 'unconventional' scientific realisms as they go against the more conventional understanding of scientific realism. These are:

- Hilary Putnam's Common Sense Realism
- Nancy Cartwright's Modelling-based Realism
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I take conventional forms of scientific realism to include a number of contentious theses which have become so commonplace that they present themselves as attitudes unreflectively accepted as given. One such attitude is that scientific realism is, at least, in tension with common sense, and, at most, has shown common sense to be false (Sankey 2020). This has taken many forms, such as Arthur Eddington's two tables (1927) and Wilfrid Sellars's (1963) attempt to reconcile the 'manifest image' with the 'scientific image'. Another attitude of conventional realism, which in truth is a symptom of the entire scientific realism debate in its current form – I say more on that blow, is that it operates within a theory-based framework. Despite the many nuances that contemporary forms of conventional realism bring, they predominately take 'theories' as their starting point, i.e. as their units of philosophical analysis. Relatedly, the last feature is not itself a thesis or an attitude but an overall deficiency of conventional realism, namely that despite paying much lip service to scientific practice, conventional realism has thus far failed to furnish an epistemology of science that takes practice as constitutive. Such lack guarantees a distorted understanding of the dynamics of scientific success production, leading to simplistic success-to-truth inferences of the kind we see on offer. Each of the three unconventional scientific realisms to be discussed challenges at least one of these attitudes, thereby liberating scientific realism and offering ways to take the realism debate forward.

At the outset it is helpful explain what I take the scientific realism debate to be. Characterizing the debate hinges on what 'scientific realism' is taken to be. But, as Ernan Mcmullin notes, "there seem to be almost as many scientific realisms as there are scientific realists" (1985, 57). This makes characterizing the debate tricky.

Fortunately, Anjan Chakravartty notes that underlying the diverse characterizations is a common core, namely that scientific realism is a positive epistemic attitude towards the outputs of scientific investigations, particularly theories (2013). So the debate could then be understood as revolving around the epistemic status of theories. But this characterization is

also skewed in favour of theories. A less committal, if also broad, characterization of the debate can be found in Cartwright (2018) who takes the central question of the debate to be "what image of the world makes intelligible the successes and failures of our theoretical practices?" (165). I elaborate below.

Cartwright (2018) is a contribution to a recent *Spontaneous Generation* issue on "The Future of the Scientific Realism Debate". What this issue shows is that many players in the debate are no longer content with limiting the debate to the traditional questions of the truth of theories and reality of theoretical entities, central these though they may be. Also, they are no longer content with confining the debate to the epistemic status of scientific output without paying attention to the process by which we arrive at these output, i.e. scientific practice, and without locating these outputs within the broader scheme of things. For example, Theodore Arabatzis takes it that a new challenge in the realism debate should be to make sense of *successful scientific practice* that was centred on entities that are thought to no longer exist (2018). Here the challenge is not in explaining the empirical success, rather in explaining the very practices responsible for it. This, to be sure, is a challenge for both realists and antirealists.

Also, when Cartwright takes the debate to be about the image of *the world* that our theoretical practices give, or when Curtis Forbes takes it to be about making sense of *reality* through the lenses of science (2018, 2), the debate, it becomes clear, is gaining a metaphysical overtone that it lacked in its formative stages. Such metaphysical traction has led commentators such as Jeffrey Foss to remark that "in the not-too-distant future the scientific realism debate will be absorbed into the far more ancient-and-venerable, old-and-unqualified, realism debate" (2018, 26).

So against the traditional questions of "is scientific theory T true?" and "does theoretical entity E exist?", the more pertinent question, which I will call the 'metaphysical question' becomes, particularly for realists, "what is the world like given our successful theoretical practices?". This question obviously is related to the traditional questions but does not reduce to them. In fact, if we manage to satisfactorily answer the traditional questions the latter question remains unanswered. For the world contains many things besides theoretical entities. Hence, answering the question of what the world is like given our successful theoretical practices requires in addition that we answer the question of how these entities relate to other entities. This question is dubbed metaphysical, as, in line with one characterization of metaphysics, contributes to the task of providing the most general description of reality.

In line with these, and similar, recent developments, I take the scientific realism debate, broadly construed, to include many questions, including not only classical once pertaining to the truth of theories/claims, the existence of theoretical entities and the status of scientific laws and principles, but also the connection between scientific practice and its outputs and how such scientific outputs to which we ascribe a positive epistemic status fit within the

broader scheme of things. This characterization will help to understand how each of the three unconventional form of scientific realism is a contribution to the debate. I now provide a brief summary of each of these positions. I note here, that as becomes clear from my more detailed descriptions in this thesis that my three central protagonists address issues in scientific realism by proposing that the world that science studies is not as conventional realists take it to be. Putnam and Cartwright are explicit about what the world is like. Putnam says it is made of states of affairs and not things displaying features. Cartwright says that things are of huge variety of different kinds with huge variety of different behaviours and features that are not literally subsumable under any reasonably small number of generalizations. Chang does not commit to what the world is like arguing that it is beyond us. In the face of this, our best notion of truth, he claims, cannot be about literally true descriptions of the world as it is but is a pragmatic one about what best guides successful practices.

It is common place amongst Putnam scholars that he went through three phases, a physicalist phase (Oppenheim & Putnam, 1958), an 'internal realist' phase (Putnam 1981), and a 'natural or pragmatic realist' phase (2000) which he maintained until the end of his life. As will later be clear, Putnam, throughout all three periods, continued to espouse what he called 'convergence realism' about science, characterized in terms of the three theses of reference, truth and convergence – this will be discussed in section 3.2., which is a pretty conventional form of realism. Why is it then treated otherwise here?

The answer to that lies in that Putnam's journey "from realism all the way back to realism", as succinctly put by Maria Baghramian (2008), helps to locate scientific realism as properly belonging to metaphysical realism – the latter is minimally construed as the thesis that the there exists a mind-independent world which we can meaningfully talk and make truth-claims about. Nowadays there are claims such as that scientific realism is devoid of a metaphysical component (Corti 2020), or that scientific realism and metaphysical realism are 'logically independent' (Alai 2020). What is relevant about the later Putnam is that in developing his common sense realism, he is not only defending a position *in* the realism debate but he is also, albeit implicitly, redefining the very debate.

Putnam's transition from his middle phase to his later phase is best understood as a rejection of deflationist accounts of scientific realism – including his internal realism which we will be discussing in Chapter 3, by emphasizing that for *any* form of scientific realism *qua realism* to get off the ground it must accept a mind-independent world to which we have both cognitive i.e. semantic and epistemic, access. That is it needs to accept that we are able to meaningfully talk about this mind-independent world and to make truth-claims about it as well.

Given that such access, for Putnam, can only be through *direct* perception for reasons that will be explained in Chapter 1, then this imposes certain constraints on what a viable scientific realist position can be. That is because if perception provides direct cognitive

access to the mind-independent world then this world as presented to us in perception is real with both its objects and properties –this highlights the metaphysical dimension of direct perception. As such, scientific realism, which also purports that science makes truth-claims about the world, is then forced to confront the important question concerning the relation between these two descriptions of the world, i.e. between the world as directly perceived and the world as described by science. This is none other than Wilfrid Sellars's (1963) question concerning the relation between the 'manifest image' and the 'scientific image' which once waxed but has currently waned in contemporary philosophy of science. The later Putnam puts this question front and centre for realists while providing a conceptual machinery to deal with it.

One may wonder why Nancy Cartwright's (Chapter4) view figures as a novel and unconventional form of realism. After all, she has been a notable player in the scientific realism debate for more than thirty years, even playing a key part in shaping the very debate. Yet that she is often taken to be either an instrumentalist or an entity realist shows that the depth of her criticism is yet to be properly appreciated. That is because though Cartwright can be understood as on the one hand an instrumentalist, and hence anti-realist, and, on the other, an entity realist, we can only regard her thus given a theory-based framework. Given this framework, one can argue: "if she opposes universal laws and theories then she must be an instrumentalist, but if she also accepts entities then she must be an entity realist". It is my contention that Cartwright's long-standing opposition to theories all the while defending realism should give us strong reasons to think that she was never merely providing another position *in* the realism debate, rather that she is criticising the very framework in which the debate has been carried out all along.

Alternatively, Cartwright who is a philosopher of scientific practice, offers local, system specific, models as her preferred unit of analysis given her observation that success in practice rarely is derived from theories. The latter is only one component in the building of local models which are the rightful loci of the source of scientific success. This conceptual shift helps to see Cartwright's views in and on the realism debate in a whole new light.

The most overt criticism of the current framework in the realism debate in recent times comes from Hasok Chang (Chapter 5) and his pragmatic realism, which is interesting in many ways. Starting from the problem of the framework, Chang shares Cartwright's dissatisfaction with theories, but goes one step further than Cartwright by criticising the 'proposition' as unit of philosophical analysis. Chang's reasons for that is that a proposition-based framework fails to capture all the non-propositional aspects which are constitutive of science, such as experimentation and other forms of non-verbal activities.

Alternatively, Chang offers a practice-based framework in terms of epistemic activities and systems of practice. Given this framework, Chang develops an epistemology of scientific practice which, having dethroned propositions, demotes truth and gives it the backseat to 'operational coherence', the notion around which the whole of Chang's pragmatic realism

revolves. Chang's realism is dubbed 'pragmatic' because, in line with the founding fathers, C. S. Peirce, William James and John Dewey, it emphasizes the central role of agents, their aims, and actions in scientific practice. It also subscribes to a pragmatic theory of truth which piggybacks on agents' actions and their operational coherence. One notable conclusion that follows from Chang's pragmatic realism and his pragmatist theory of truth is that what appears to be conflicting theories with different ontology may both be true without inconsistency. So, for instance, on Chang's account, both phlogiston and oxygen can be real given different systems of practice.

This thesis has a direct aim stated at the beginning, namely to provide a critical exposition of these three positions in the hope to highlight some interesting features of each position, some of which are foreshadowed here in the introduction. But it also has a wider, but more indirect aim, namely to provide groundwork for a synthesis of these three positions which brings together the strengths of each position while avoiding its weaknesses.

These three positions, to be sure, are incompatible on many levels. From the very beginning they disagree on the proper unit of success, with Putnam emphasizing theories, Cartwright models, and Chang epistemic activities. Also Chang's new framework with its new notions of 'pragmatic truth' and 'pragmatic reality' makes his position incompatible with both Putnam's common sense realism and Cartwright's modelling-based realism. Worse, Putnam's emphasis on the necessity of cognitive access to the mind-independent world makes Chang's pragmatic realism, which denies such access, not even a form of realism on Putnam's account.

Nonetheless, there is something that brings together these philosophers at the meta-level, namely that they are not merely offering another realist position to be added to the list, but they are also offering important insights which, if assimilated, would transform how we ought to understand scientific realism. Particularly, Putnam's bringing back scientific realism to metaphysical realism, Cartwright's model-based particularism, and Chang's epistemology of practice with its notion of operational coherence which helps to highlight the material component of scientific success, otherwise passed over in more abstract theorizing about scientific realism. I argue that their three proposals are all compatible. I conclude by providing some groundwork for a scientific realist position that integrates their diverse insights into a synthetic whole.

I think of constructing such a synthesis by taking Putnam's common sense realism as a broad metaphysical framework and accepting Chang's epistemology of practice. The latter would have to be given a stronger realist reading by interpreting Chang's 'operational coherence' as a theory of epistemic justification while reinstating truth in the strict realist sense. Cartwright's model particularism can be easily accommodated on this new reading of Chang's epistemology. This can be done by taking a page out the selective realists' book and breaking down what is often called the unit of scientific success – which figures in success-to-truth inferences, into 'unit of analysis' and 'unit of commitment'. Almost all selective

realists nowadays agree, albeit implicitly, that the unit of analysis is 'theory'. What they are selective about, however, is the unit of commitment. Some of them choose structure, others entities, etc. In the same vein, we can, following Chang, accept epistemic activities and systems of practice as units of analysis all the while maintaining Cartwright's local system-specific models as units of commitment. When an activity in which the model plays a central role is deemed operationally coherent, the corresponding model would be said to faithfully represent the local, mind-independent states of affairs, and from it we can construct local claims that are true in the strict realist sense.

I would like to close this introduction with a few remarks on the style of writing in the dissertation. The reader will notice different styles in different chapters. That is because I have approached each chapter with a different aim in mind and had, accordingly, to choose a style that befits each aim. Some of these chapters are more exploratory such as chapters 1-3 on Putnam. Others are more direct albeit still broad such as chapter 4 on Cartwright. Finally, chapter 5 on Chang is the most detail-oriented with eye to the overall position. I will briefly explain the reason for these variations.

Putnam once noted that his later position is underappreciated and underexplored. I took that remark to heart and decided to *explore* his later view, hence the style of the chapters. It is worth noting that pinning down Putnam has proven to be the most difficult aspect of this dissertation for two reasons.

First, Putnam continuously introduces and retracts a battery of new concepts without sticking to them. For instance (Chapter 1-2) Putnam describes his position as 'common sense realism', but he also describes it as 'natural realism', elsewhere he calls it 'pragmatic realism' and other places he uses 'realism of common man'. This example is perhaps benign as in this case Putnam is arguably referring to the same position which I ended up calling 'common sense realism' to avoid confusion.

However, this proves problematic in other cases. Consider for example Chapter 3, where I discuss Putnam on scientific realism. There, I note that Putnam used the term to denote three very different things, namely materialism, convergent scientific realism, and metaphysical realism. Another example is where, in the same chapter, I point out that he uses 'metaphysical realism' in two different senses. He begins by rejecting 'metaphysical realism' only to finally accept 'metaphysical realism' but the latter has a different meaning than the former. I have tried to clarify these differences in the discussion. The problem with these is that unlike in the case of 'common sense realism' the difference between these does change the meaning of claims that Putnam is making and causes a lot ambiguity while trying to interpret him.

Second, Putnam is a prolific writer who is known to have changed his mind often. I am interested in discussing Putnam's *last* views which do not constitute an entire break with his former views. In fact, as will be clear throughout my discussion, Putnam continuously refers

to and draws on arguments he made previously and assumes his reader is already familiar with them. This required that I provide explanations of these arguments which may distract the reader form the original point being made. As a remedy I try to include a lot of signposting to make sure the reader is not lost in the details. By that I am sacrificing smoothness for clarity.

In chapter 4 on Cartwright the style is more direct but somewhat still broad in approach. There I did not aim to engage too much with the details of Cartwright's arguments for what is called instrumentalism about theories and laws and realism about entities, for I already accept these arguments and their conclusions. My aim, which I also take to be the most important contribution of this chapter, is to unify these diverse arguments and local views under a more global position which I call Modelling-based Realism. Hence the chapter turned out shorter but much denser than the first three chapters, if also somewhat broad.

In chapter 5 on Chang, the style is also direct but here it emphasizes both breadth and depth, i.e. I engage with Chang's work, going down to the smallest details all the while paying attention to the general framework. This is also, arguably, the most critical chapter. Having been immersed in Chang's work I took on board much of its lessons, but I was on the whole dissatisfied with what I took to be an antirealist undertone. As such, my aim in this chapter was twofold: first, to correct what I took as an important weakness in the notion of operational coherence and, second to give Chang's practice-based framework a stronger realistic twist.

Finally, as the discussion of the later Putnam ranges over three chapters, I find it fitting to start with a general introduction that locates each chapter within the broader 'the later Putnam on realism project', and to close with a general conclusion that ties these chapters together. The following is the general introduction.

Putnam's Common Sense Realism: a General Introduction

It is now commonplace amongst Putnam's scholars and commentators to claim that Putnam has gone through three phases, a physicalist phase (Oppenheim & Putnam, 1958), an "internal realist" phase (Putnam 1981), and a natural/pragmatic/common sense realist phase (2000) which he maintained until the end of his life. What I am interested in is the later Putnam's common sense realism, which he described as "Aristotelian realism without Aristotle's metaphysics" (2000, 4). Common sense realism, in the spirit of Aristotle as well as John Dewey, is a form of realism that pays dues to common sense while being squarely in touch with the results of science ((2004a, 66).

Putnam's common sense realism comprises two components.

- A defence of direct realism in perception
- A defence of metaphysical/scientific realism.

The need for these two components is due to a point recurringly stressed by the later Putnam, namely that one cannot have any form of scientific realism without already securing cognitive access, i.e. semantic and epistemic access to the mind-independent world. This, for the later Putnam, can only be done by adopting the right account of perception, namely a form of direct realism.

Direct realism can be understood either as a *genus* term denoting all and only positions according to which veridical perceptual experiences essentially consist of relations to ordinary mind-independent objects and their features, and it can be understood as a particular *species* belonging to this genus, i.e. a particular account of direct perception. Putnam's project involves both meanings. Chapters 1 and 2 deals with each in turn. This will eventually pave the way for chapter 3 where I discuss the later Putnam's views on scientific realism.

Chapter 1 is concerned with Putnam's defence of the genus direct realism. Before getting to that, I introduce the later Putnam's motivation for common sense realism, explaining why he thinks a question in epistemology, i.e. on the nature of perception, is relevant for metaphysical and, accordingly, scientific realism. As explained in section 1.2., the reason for that is that the middle Putnam had devised a model-theoretic argument that renders reference radically indeterminate for any philosophical position that accepts an indirect/representationalist view of perception. This leads the later Putnam to conclude that the only way to maintain realism is to accept a direct view of perception which secures cognitive access to the mind-independent world. This requires that the genus direct realism be defended against the challenges raised against it by early moderns and more contemporary philosophers. These challenges can be divided into epistemological and metaphysical, and Putnam takes on each in turn.

In section 1.3., I explain how Putnam deals with the epistemological arguments against direct realism, particularly arguments from illusion, hallucinations and dreams. As these phenomena are thought by many to be better accounted for on an indirect, sense-data account, of perception, Putnam confronts sense data theories of perception arguing that 'sense data' are utterly mysterious entities or processes and attempts to clarify them face severe difficulties. In section 1.4., I turn to Putnam's responses to metaphysical challenges to direct realism, particularly those that trade on the distinction between primary and secondary properties, where the former but not the latter are said to belong to the mind-independent world. Against that, Putnam provides a reinterpretation of the 'Berkeleyian' arguments from inseparability and perceptual relativity to the effect that, granting the distinction, both primary and secondary properties belong to the mind-independent object. Putnam's ultimate conclusion, as summarized in section 1.6., is that the epistemological and metaphysical challenges are not as severe as proponents of indirect realism claim. They are not strong enough to shake our trust in genus direct realism as a broad theoretical framework to deal with the question of perception.

In Chapter 2, I turn to Putnam's defence of the species direct realism, i.e. his own account of it. This Putnam does while engaging with John McDowell's own species of direct realism. As such, in section 2.2., I lay out McDowell's position which can be characterized in terms of four theses: 1) Openness, 2) Perceptual Disjunctivism, 3) Minimal Empiricism, and 4) Perceptual Conceptualism. Then I expound Putnam's views by explaining his take on each thesis.

I do not discuss Putnam's take on openness because it denotes none other than what I have called the 'genus direct realism', which I already established that Putnam accepts, and I dedicated the entire chapter 1 to explaining his defence of it. In section 2.3., I explain why Putnam rejects disjunctivism –the position according to which in veridical perception there is no such thing as sense data. In section 2.4., I explain why Putnam rejects perceptual conceptualism – the position that all sensations presuppose our conceptual powers. I will not need to discuss Putnam's view on minimal empiricism – the position that sense impression is what ultimately justifies perceptual beliefs, because Putnam accepts with McDowell that whatever is supposed to provide the basis for empirical judgment must be conceptualized, and his rejection of perceptual conceptualism implies that it cannot be 'sensation'. In section 2.5., I introduce a key component in Putnam's account, namely 'apperception', and I explain the crucial role it plays in his direct realist position.

In chapter 3, I turn to Putnam on scientific realism. Putnam uses the term to denote three different things: scientific realism as 'as convergence', scientific realism as 'metaphysics' and scientific realism as materialism. I concern myself with the first two meanings. In section 3.2., I explain scientific realism as convergence, which is characterized in terms of reference, truth and convergence. In section 3.2.1., I explain Putnam's now famous no miracle argument in favour of convergence realism. In section 3.2.2., I explain the challenge raised

against convergence realism, particularly in the form of the incommensurability thesis. In section 3.2.3., I consider Putnam's response. In section 3.2.3.1., I explain why Putnam's *convergence* realism ultimately fails.

In section 3.3., I explain the second meaning of scientific realism, which is scientific realism as metaphysics, what Putnam also sometimes calls metaphysical realism. The latter is characterized in terms of ontology, ideology and correspondence. In section 3.3.1., I explain why Putnam rejects metaphysical realism. It will turn out that Putnam only rejects one kind of metaphysical realism for another. In section 3.4., I explain the latter kind. This includes two components, one phenomenon called conceptual relativity which I discuss in section 3.4.1., and the other called conceptual pluralism, which I discuss in section 3.4.2.. These discussions will pave the way for a new take on the relation between science and common sense, which I take up in section 3.4.3.. I then provide a general assessment of Putnam's scientific realism section 3.5., before providing a conclusion to chapter 3 in section 3.6.. This is then followed by a general conclusion that will tie the discussions of all three chapters together.

Chapter 1: Putnam's Direct Realism: a Defence of a Framework

1.1. Introduction

This chapter is concerned with the first component of Putnam's common sense realism, namely his 'direct realism' in perception. Particularly, I will focus on the later Putnam's defence of the *genus* direct realism against early modern and more contemporary challenges.– In the next chapter I will be concerned with expounding Putnam's own species of it.

Given that my bigger goal is to explore the later Putnam's common sense realism, I begin in section 1.2. by providing some stage setting which helps to explain why Putnam thinks that a question in epistemology, i.e. on the nature of perception is relevant for metaphysical, and, accordingly, scientific realism. Then, I turn to his defence of direct realism against the challenges raised by early moderns as well as the more contemporary philosophers. These can be divided into epistemological and metaphysical challenges. I explain Putnam's response to both in turn.

In section 1.3., I consider the epistemological challenges, which are the classical arguments from illusions, hallucinations and dreams. Such challenges for Putnam, trade on the claim that indirect realism in the form of sense data theories is able to better explain these phenomena and provide, overall, a better account of perception than direct realism. Hence, a significant part of Putnam's defence of direct realism is his attack on immaterialist as well as materialist versions of sense data theories. In section 1.3.1.1., I explain Putnam's challenge against immaterialist versions of sense data theories which take sense data to be products of brain events. Putnam briefly discusses these views because they are no longer as popular as they used to be and he is more concerned with challenging materialist versions which are much more common nowadays. In section 1.3.1.2., I explain his challenge, to materialist versions of sense data theories, particularly those that espouse an identity theory of the mind. But Putnam does not stop at that. In section 1.3.1.2.1., I explain how he goes on to problematize the very *meaning of identity* at play in these positions. In section 1.3.1.2.1.1., I consider his response to the suggestion that identity is sui generis. In section 1.3.1.2.1.2., I consider his response to the suggestion that identity is theoretical identification. In section 1.3.1.2.1.3., I consider his response to the suggestion that identity is anomalous token identity. Putnam finds all of these, and ultimately, the epistemological challenges wanting.

In section 1.4., I consider Putnam's response to the metaphysical challenge against direct realism. This challenge trades on the distinction between primary and secondary qualities, where the former but not the latter are said to belong to the mind-independent world. Putnam responds to this challenge with two Berkeley-type arguments, and I explain these in turn. In section 1.4.1., I explain the argument from inseparability to the effect that the so-

called primary and secondary qualities cannot be separated. Berkeley used this to argue that both belong to mind, Putnam uses this to argue that both belong to the world. In section 1.4.2., I consider the argument from perceptual relativity which says that whatever is true of perceiving secondary qualities is true of perceiving primary qualities. Again, Berkeley used this argument against Locke to argue for idealism. Russell used it to deny the reality of both primary and secondary qualities. Putnam uses it to defend the reality of both primary and secondary qualities, although, to reach that, he notes that we should accept a more sophisticated conception of 'qualities', and shows how this could be done in the case of colour.

In section 1.6., I summarize the lessons taken from Putnam's defence of direct realism, namely that challenges raised against it are not as severe as its opponents claim, that sense data theories are no more scientifically respectable than some theories of direct realism and that ultimately we have no good reason to give up our native epistemic position of direct realism.

1.2. Preliminaries

Early modern philosophers, Putnam explains, as well as contemporary cognitive scientists, have, for the most part, accepted a Cartesian framework, wherein the object of immediate experience is not the physical object itself but a non-physical, mental image or representation of it. These representations, we are told, are linked causally, although not cognitively to the objects in one's environment. That is the world causes in us these representations with no guarantee of the latters' veracity. This, thus, creates a schism between mind and world, for if our only access to the world is through representations whose veracity is in question and if our cognitive powers can operate only given these representations, then on such views, our minds can never stretch out to reach the objects. Instead, they can only connect to them through an intermediate (2000, 10).

This false conception of perception, Putnam concurs with John McDowell, has disastrous consequences for "about every part of metaphysics and epistemology" (10). Some problems in philosophy seem unsolvable, Putnam holds, due to the fact that all philosophical candidates on offer presuppose "a very broad, if vague consensus on the nature of perception" (12). To see that, consider one of the most polarizing problems in philosophy, namely the problem of realism.

The enduring question in analytic philosophy of "how does language hook on to the world?", Putnam remarks, is the same old "how does perception hook onto the world?" (12), however with a more linguistic garment. For if we cannot secure cognitive acess to the world, which include semantic and epistemic access, then trying to secure linguistic access is of no avail. Thus, he contends that failing to adopt the right theory of perception makes of realism a "giant antinomy of reason" (13). Here is how the antinomy comes about.

Putnam begins with common assumption that the mastery of a language consists in its 'use'. This, he notes, can have an early modern reading where 'use' is used to denote of disposition to respond mental representations (15), but it can also have a more updated, 'computer scientific' reading, where use is understood, as Putnam puts it, as 'running a computer program in the brain' (14). In both cases, our mastery of language, i.e. our use of the language depends on the external environment. Particularly, to single out an interpretation of our language this will depends on having a description of the external causes of the language users' words. But if our access to the external world is causal but never cognitive – that is assuming an indirect view of perception, then the question of how terms begin to have denotations become prominent. What exacerbates this issue is that Putnam has devised a model-theoretic argument – which I will explain in chapter three, to the effect that

> if the sort of realism we have been familiar with since the early modem period, including the causal theory of perception, is right, then everything that happens within the sphere of cognition leaves the objective reference of our terms, for the most part, almost wholly undetermined (16).

Putnam himself did not embrace the radical indeterminacy of reference. His argument was introduced as a reductio ad absurdum against a particular metaphysical picture which takes that mind-independent world to possess a fixed number of objects and properties and takes truth to be a strict correspondence between language and the world –this will be further discussed in chapter three. It presents proponents of this views a picture with a dilemma which says that "either the use of the language already fixes the 'interpretation' [of our words] or nothing can"(17), thereby leading to what he believed to be an antinomy of reason. For neither of these options are acceptable according to Putnam.

The former would have to presuppose some faculty such as 'neotic rays' which is too supernatural for Putnam's naturalistic liking, and the latter leads straight into solipsism which is incompatible with realism. This conclusion, Putnam explains, applies to *any* language, including both ordinary and scientific language (16).

The Middle Putnam's solution was to adopt 'internal realism' which replaces correspondence with an epistemic theory of truth according to which truth does not transcend justification – I will also explain this transition in details in chapter three. But the later Putnam came to realize that this prima facie antinomy is simply the result of a false, but entrenched, account of perception, or more precisely a particular framework within which the question of perception is tackled.

The later Putnam calls this framework "Cartesianism cum materialism", but it can also be called indirect realism. It says that "perception involves an interface between the mind and the "external" objects we perceive". This comes in many guises. It includes both the immaterialist reading of early modern epistemologists and metaphysicians according to

which the interface consists of “impressions”, “sensations”, “sense-data”, “qualia”, etc., as well as the nowadays commonplace materialist reading according to which the interface consists of brain processes (43).

Putnam’s starting point is not agnosticism about the right framework to deal with the question of perception, i.e. he does not start by suspending judgment on whether we should adopt direct or indirect realism. Instead, he takes direct realism to be what I will call our “native epistemic position”, or what he calls following William James, “the natural realism of common man” (24). That is the starting point of common man is direct realism.

Putnam emphasizes that the view that we can only experience events that are internal to ourselves, events whose only relation to the external objects is that of being caused by them, is foreign to the philosophical tradition prior to the early modern period (22). He then proceeds to criticise the arguments that have been forwarded by the early modern philosophers to shake our trust in direct realism. These are of two kinds: an epistemological kind which concerns the similarity between veridical and nonveridical experience, and a metaphysical kind which concerns the different status of what we take to be the properties of external objects. We deal with these in turn.

1.3. The Epistemological Challenge to Direct Realism

Putnam considers the now famous arguments from ordinary illusions, hallucinations and dreams, which were put forward in order to establish that we have sensory experiences which are not veridical, and asks, granting that it is in fact the case that we have some non-veridical sensory experiences, what are we to infer from that?

Putnam’s answer is simply that perception is not infallible. However, it is a long way to infer from “perception is not infallible” to “therefore it cannot be direct” – which is the conclusion the argument is meant to establish, without adding further contentious premises. Also, consider illusions such as the stick’s looking bent in water, or the mirror image’s being mistaken for the object itself. Putnam remarks, in these cases, I am already perceiving things which are *‘out there’* which *looked* like something else and as such I *took it for* something else (25-26). That is there is an intuitive *directedness* in perception, where things that are perceived are taken to be *outside* the mind.

Putnam then pays close attention to the case of dreams. He notes that although we may have some vivid and rather realistic dreams, the experience we have in them is always of *as if* we were seeing, hearing, etc. What Putnam is trying to point out, following J. L. Austin, is that the *phenomenology of dream experience* is different than ordinary perception. But what if someone claims to have a dream which is qualitatively indiscernible from ordinary perception? Putnam is right to point out that we cannot dismiss that option a priori, yet maintain that granting that does little to undermine direct realism, for the following reason.

1.3.1. Putnam against Sense Data

Consider the following thought experiment. Helen has a dream experience of being in front of the Taj Mahal. Helen has never been to the Taj Mahal before. Her dream experience is so real that it is qualitatively indistinguishable from actually being there. For a sense datum epistemologist, Helen is experiencing *something*, Putnam explains, which is obviously not the Taj Mahal. What could it be? The answer is that Helen is experiencing 'something mental'. As such, we are led to conclude that at least sometimes our immediate object of perception is something mental.

Next, imagine that Helen actually went to India, and this time she actually had "exactly the same visual experience". It is not implausible to conclude, the sense datum epistemologist notes that

> On the second occasion she was indirectly perceiving the Taj Mahal and on the first occasion she was not even indirectly perceiving it, but what she immediately perceived on both occasions was her sense data (27).

However, Putnam notes that this argument assumes a number of premises which are accepted rather gratuitously. These include that

1. The dreamer is actually *perceiving* something.
2. Whatever is not physical must be mental.
3. Talk of "direct" and "indirect perception" makes sense.
4. Dreaming and waking experiences are qualitatively indiscernible.
5. Object with radically different natures cannot *appear* exactly alike (27).

Putnam, remarks as did James and Austin before him, that even if we grant the first four of these assumptions, there is no reason that we should grant 5 and if 5 cannot be supported, then the entire argument from dreams, illusions, etc. is a non-starter. Putnam suggests a way to salvage the argument which he also finds unsatisfactory and we consider it in what follows.

According to Putnam, the sense datum epistemologist may concede that her argument is not 'deductively certain' – since she has, at least, not given any reason for accepting 5. She may also acknowledge that her theory is unsatisfactory as a description of the phenomenology of ordinary sense experience because ordinary people do not talk of sense data when describing their experience, sense data belongs to the philosophical jargon and not ordinary language.

Nonetheless, she may hold onto the theory not as a description but as *the best -available-explanation* for a number of facts pertaining to perception. Going back to our aforementioned thought experiment, she may hold that even if Helen's dream and ordinary

perception are not qualitatively indiscernible, direct realism renders the fact that there is *similarity* between these two experiences mystifying. Conversely, sense data theory can explain this fact by holding that in both cases what Helen was perceiving was none other than her sense data (29). Thus, sense data theory seems to provide the best explanation for the similarity that exists between two, presumed, very different kinds of experiences. Putnam notes that this rejoinder can be understood in different ways depending on whether we adopt a materialist of an immaterialist understanding of sense data. Putnam takes on each in turn.

1.3.1.1. Against the Immaterialists

The immaterialist version, Putnam explains, says that sense data are simply products of brain events. But Putnam finds that this response to be unsatisfactory. He notes, particularly, that sense data theory in its immaterialist version papers over the similarity between veridical perceptual experience and dreams but does not explain it.

For the Cartesian epistemologists, according to Putnam, have provided *not one* mechanism explaining *how* events in the brain produce sense data or how the mind is able to 'immediately observe' posited objects (29). Putnam highlights that sense datum epistemologist's agreement is also almost only terminological. They agree on "sense data". Yet, they disagree on what they are supposed to be. For some, sense data are parts of the mind, for others, they belong to the mind without being part of it. Some claims they are particulars, while others hold that they are qualities. These considerations lead Putnam to conclude that an explanation of the similarity in experience in terms of sense data is

> [An] "explanation" in terms of utterly mysterious entities and processes – one that lacks all detail at just the crucial points and possesses no testability whatsoever, [which] would not even be regarded as intelligible in serious natural science (29).

1.3.1.2. Against the Materialists

The materialist version, Putnam explains, unlike its immaterialist counterpart, does not claim that sense data are *products* of brain events. Instead, they hold that sense data are *identical* with brain processes or events. This is known as the 'identity theory' of the mind. The advantage of this view over its immaterialist counterpart is that it is able to circumvent the problem of accounting for how brain events cause mind events by claiming that these prima facie events are in fact identical. Yet, holds Putnam, identity theory is unable to explain how we become *conscious* of qualia. Here, Putnam is flagging what has come to be known as the "hard problem of consciousness".

To illustrate, Putnam considers Jerry Fodor's view, who, in the *Modularity of Mind* (1983), terms the output of the "perception module" "appearance". This assumes, Putnam explains,

that “when one of these hypothetical pattern recognizers produces "outputs" the event is ipso facto a conscious event” (30). Yet this brings important difficulties. Putnam considers the case of “blindsight” which is “the capacity of some individuals with damage to the striate cortex (primary visual cortex or area V1) to detect and even localize visual stimuli presented to the blind portion of the visual field” (VandenBos, 2005, 113), and the case of “split brain” which is “a brain in which the two cerebral hemispheres have been separated by partial or complete destruction of the corpus callosum” (1019). Patients with either of these two conditions are able to respond to some stimuli without being conscious of them. To use Fodor’s terminology, in both cases we can have perceptual output, i.e. appearances, without consciousness, thereby contradicting Fodor’s identity claim of qualia with appearance.

Putnam also puts forward the following thought experiment. He propose a scenario where we managed to reach a level of technological advancement wherein we are able to isolate modules that are involved in visual recognition of, say, a ‘chair’ and assuming that we managed to keep it alive and functioning in a vat. If we manage to make it respond to stimuli, as it normally does, are we then to say that the module is generating qualia even with no person there to experience it? This for Putnam sounds absurd.

Putnam notes one final response that may be attempted by the materialist to salvage the case for identity theory. That is to search for a ‘grand neuron’ that somehow *transforms* messages in the visual cortex into qualia. Such an attempt, Putnam notes, is blocked because although brains have visual centres, speech centres, etc., they do not have a *centre for consciousness*. This fact, he suspects, is what led some materialists such as Daniel Dennett (1993) to deny both the reality of subjective consciousness and objective reference (31). All these considerations lead Putnam to hold that the materialist version of identity theory also fails as an explanation for the ‘immediate perception’ of sense data. Next, Putnam highlights a more fundamental problem with identity theory, namely what “identity” is supposed to capture in this context.

1.3.1.2.1. On the Meaning of Identity

To this question, Putnam provides three possible answers.

1. Identity is sui generis.
2. Identity is theoretical identification.
3. Identity is anomalous token identity.

1.3.1.2.1.1. Identity as Sui Generis

Consider the first option. By holding identity as sui generis, Putnam holds, identity theory is not different from “psychophysical parallelism” which is “a form of dualism according to which mental and physical processes are perfectly correlated but not causally connected”

(Colman 2015). For both end up postulating something that is sui generis. It is 'identity' in the former case and the correlation that holds between the mental and the physical in the latter (Putnam 2000, 32). Despite the prima facie differences, Putnam adds, these two views are not mutually exclusive, and may even amount to the same thing, and no identity theorist, he notes, would want to subscribe to a view that makes room for dualism.

1.3.1.2.1.2. Identity as Theoretical Identification

As for the second option, it is the view favoured by his former self but which he later came to reject. In *Reason, truth and history* (1981), Putnam distinguished between two notions of sense data (or qualia):

1. Functional.
2. Qualitative.

'Functional sense data' means the sense data that one has by public standards. Such as having a sense data of red when you are observing something that everyone agrees is a red apple. As for 'qualitative sense data', in our example, it is whatever quality I *privately* associate with the word "red" irrespective of everyone else's association. The following thought experiment will help further clarify these notions.

Putnam invites us to consider the case of "inverted spectrum" (80). In this case, for a particular person, blue things appear red to her and vice versa. Yet this person has been taught, since her infancy, to use language in a way that whenever she sees what appears red to her, she utters the word "blue", and her uttering "blue" would be right according to normal people who would also utter "blue" when something that appears blue to them. The same, accordingly, goes for the colour red. Now if we introduce the two notions of sense data, we would say that the person, when seeing what in fact is blue, has a functional sense datum of blue which is the same functional sense datum that normal people that look at blue things would have. However, when it comes to the qualitative sense data, in this case, the person has a qualitative sense datum of *red*, which is different from the qualitative sense datum of normal people which is in this case blue.

1.3.1.2.1 2.1. Functional Identification

Now granting that sense data are brain states as the early Putnam and many identity theorists do, then it becomes the case that when grouping sense data according to their functional characteristic – which in this case are computationally characterized brain states, we are using the functional notion, and when grouping sense data according to how they appear to us – which in this case involves certain neurological characteristics, we are using the qualitative notion.

What the early Putnam did here is that he clarified two distinct notions of sense data prior to making the identity move. What remains is to make the claim, as many identity theorists do, that the term "sense data" belongs to a scientific theory, and that that scientific theory can eventually be reduced to physics or physic-cum-computer science through the use of theoretical identification (2000, 32). The problem for the identity theorist however, notes Putnam, is that we have no such theory on offer for either sense of the term.

But as we mentioned earlier the term "sense data" is itself a philosophical notion which does not figure in ordinary language. People may speak of "seeing", "hearing", etc. as describing sensations but not sense data. We may provide a theory that identifies sensations with sense data, as such we may regard beliefs about these as a belief about something *looking like* or *seeming like* something else (33). But doing that, Putnam remarks, we would be really shoehorning a theory into our pretheoretical understanding for no scientifically good reason. For the theory introduced here yields neither prediction nor explanation.

But even if we grant that a theory that fits the bill can come about in the future, Putnam holds, such theory would have to at least cohere with our web of ordinary psychological beliefs. That at least it should cohere is because for the theory to be successful it needs not to reduce the entirety of psychology to physics cum computer science. It needs only to reduce sense data while merely cohering with the rest of the web of ordinary psychological belief – those that do not include or are not connected to sense data. However, even this minimalist aim brings with it a very difficult problem, that of reducing intentionality to physics cum computer science. The reason for that will be made clear in what follows.

Putnam notes that our ordinary web of psychological beliefs includes:

1. Intentional notions.
2. Propositional attitudes.

He notes that there is a tight link that exists between sensation concepts (i.e. seeing, hearing, etc.) and propositional attitudes. For example, when I have the sensation of seeing a red apple, this is normally accompanied by *belief that* I am seeing a red apple.

Now, reducing a scientific notion to another through theoretical identification, Putnam explains, depends on showing the derivability of laws of the former science from those of the latter, with the aid of theoretical identifications, perhaps using some bridge principles along with notions that belong to the reducing theory. Putnam's favourite example is "light is an electromagnetic radiation". Yet, he is quick to point out that it is not enough to have reduced the term light, our reducing theory should also reduce other notions belonging to the reduced theory such as shadow, penumbras, reflection, etc. (33).

By the same token, in order to reduce sense data talk, it is not enough to reduce just that term. One must reduce the entire theory involved along with all other notions that figure with sense data. This is where intentional notions and propositional attitudes become prominent. For as we previously noted, ordinary talk of sensation (which became sense data when it took on a theoretical form) seems to be closely related to intentional notions as per the example above. Consequently, to reduce sense data talk one must be able reduce intentional talk, a task which, Putnam holds, is impossible.

We begin by noting that the reason why Putnam provides "light" as an example of theoretical identification in order to discuss the identification of intentionality with physics cum computer science is due to his conviction that the latter project, like the former is a synthetic, empirical one – as opposed to a purely conceptual, analytic one. That is why Putnam introduces the notion of "synthetic identification of properties" in order to clarify what he has in mind when he speaks of identification. He was not content with saying that, say, an *event* described using light-talk is identical to the same event which is described instead using electromagnetic radiation-talk. Instead, Putnam wanted the stronger claim that the *property* is the same property as being an electromagnetic radiation of a certain wavelength (Putnam 1997, 33). It is by keeping these in mind that we look at the two approaches to carrying out his reductive project.

1.3.1.2.1.2.1.1. Reducing Intentionality

1.3.1.2.1.2.1.1.1. Intentionality as Causal co-Variation

In its most basic form, the first approach suggests the identification of intentional talk with its *causal co-variation* with what it is about. One sophisticated attempt is Fodor's attempt to explain reference in terms of counterfactuals. Putnam lays down Fodor's views as follows.

According to Fodor, Putnam (1995) explains, to make an assertion that contains the English word "cat" is called "'cat' tokening". In this example, causal co-variation involves an extralinguistic thing which causes "cat" tokening, leading to the following 'law[1]':

1. "Cats cause "cat" tokenings" (36).

Keeping in mind that Fodor here is trying to explain reference in terms of causal connection between word and world, what this law is supposed to capture is not that all things being equal cats will cause "cat" tokenings, rather that cats cause "cat" tokening more than any other kind of object. The law that 1. instantiates looks something like this:

[1] Law, in this context is not to be understood as exceptionless regularities, but more ceteris as paribus. Nonetheless, for Fodor, they still support counterfactuals.

2. W causes “W” tokening, where W is the thing in the world and “W” is the word tokened when W is involved.

Yet Putnam is quick to note that, for instance in the case of (1), it is not only cats that cause “cat” tokenings, there is also the picture of a cat, a plastic cat, the sound of “meow”, etc. Fodor’s project, to be sure, is not to tell us what on a particular occasion “cat” refers to. Instead, his project, if successful, is meant to provide, what Putnam calls the “basic” meaning of type word “cat” (38). Hence, there should be that there is a counterfactual dependence amongst truths that figure in (2) and their more concrete counterparts, to the effect that, for instance in the case of (1):

3. “If cats didn’t cause “cat” tokenings, then the other things that we mentioned (cat pictures, cat statues, the sound “meow”, and so on) wouldn't cause “cat” tokenings either” (38).

Thus, what Fodor, according to Putnam, is suggesting is that there is *asymmetric dependence* amongst things which cause “cat” tokenings, with cat figuring in the basic law with other laws involving cat pictures, cat statues, etc. asymmetrically depending on it. Yet Putnam points out that Fodor’s analysis, if granted, succeeds only in establishing dependence, but falls short of *asymmetry* – which is necessary for the success of Fodor’s project. The reason for its failing to establish asymmetry will be further clarified.

If we grant for the sake of argument that a law such as cat picture causes “cat” tokening depends on the law that cats cause “cat” tokenings, this does not automatically entail asymmetry, for there is nothing in this claim which prohibits that the converse is also true. In order to establish asymmetry, Fodor must provide an argument as to why it is not the case that laws” if cat pictures didn’t cause “cat” tokenings, then cats wouldn’t cause “cat" tokenings either”(39). Putnam considers a number of possible rejoinders, and counters them with counterexamples, thereby concluding that Fodor’s causal co-varition theory fails to provide necessary and sufficient conditions for reference (41).

Hence what we have here, according to Putnam, is a failure of the first attempt to reduce intentionality to a ‘science’. This was followed by another more sophisticated approach, and we look at this in what follows.

1.3.1.2.1.2.1.1.2. Intentionality as a Computational State

Unlike the previous attempt which claims to identify intentional talk with causal co-variation with what it is about, this attempt, once defended by the early Putnam, is functionalist in nature – functionalism being a view in philosophy of mind according to which, roughly, the mind is akin to computer software run on a hardware being the brain and with mental states being functional states. That is, it attempts to identify intentional talk, such as the varieties of propositional attitudes, with computational states. These, in turn, can be made

more precise through the formalism of computational theory such as Turing formalism or automata theory. This promising project, however, Putnam notes, faces devastating difficulties and we shall see why.

Putnam explains that in a computer formalism theory, each and every computational state is implicitly defined by the totality of its computational relations to other states in the system, this makes the entire set of computational states *simultaneously implicitly defined*. That is precisely what individuates one computational state from all the rest. The problem with applying this to intentional talk is that such implicit definition of states, Putnam notes, is entirely alien to all psychological theories (2000, 34).

Take for example the case of the computational states as specified using Turing formalism. In a Turing machine, the machine can only be in exactly one state at a time. Also, memory and learning, Putnam explains, are not represented by states. Instead, these are represented by information printed on a machine's tape. Thus, if psychological states are to parallel Turing states, they must (Putnam 1997, 34):

1. Be independent of memory and learning.
2. Be totally instantaneous.
3. (Along with memory and learning) totally determine the next state.
4. Totally specify the current psychological condition of a human being.

States that satisfy these conditions, Putnam remarks, would differ drastically from both propositional attitudes as we understand them and from states as postulated by our currently available psychological theories. This last remark is crucial for the project of identifying intentionality with physics cum computer science is supposed to be, according to Putnam, an empirical one and as such should not go against the grain our current scientific understanding.

One possible rejoinder is to postulate an "ideal psychological theory" which we will eventually arrive at. Unsurprisingly, this theory would (have to) have the same sort of properties as computational theory has. But given what we have previously noted, this strikes Putnam as rather ad hoc. Worse, no one has any clue what such a theory would look like and, Putnam notes, no one knows how even to begin building such a theory.

Given the difficulties noted with both the reducing theory and the theory to be reduced, it becomes very difficult to make sense of the "notion of "identity" of "sense data" with "functionally characterized states of the brain" (35), leading Putnam to conclude that the project of reducing the entire body psychology to physics cum computer science is empty. These challenges seem to be decisive against a functionalist conception of sense data. In what follows he considers the qualitative conception to see if it fairs any better.

1.3.1.2.1 2.2. Qualitative Identification

Unlike functional identification which has difficulties at both the reducing theory as well as the theory to be reduced, qualitative identification, according to which sense data are directly physically characterized, seems to have no difficulty at the level of the reducing theory. Such theory is characterized in terms of physics and brain chemistry. But what about the theory to be reduced?

Given that qualitative identification, like functional identification falls under the rubric of theoretical identification, Putnam's earlier remarks still apply, especially that it is not enough, for identification to succeed, to reduce one notion while disregarding closely related notions. That being said, Putnam considers the example of "visual sense data" (36). A theory that reduces visual sense data would have to reduce the relation amongst colours. Taking note of Jerry Lettvin's (1959) theory of visual perception – according to which much computation takes place in the eye and not the brain, Putnam remarks, there are at least two ways to do that.

1. Identify colour relations with processes in the eye, or
2. Identify colour relations with processes in the visual cortex.

Yet remarks Putnam, it cannot be the case that 1 because one that has lost both his eyes still has colour sensations. This, according to Putnam, suggests that the identification of sense data with chemical and/or physical processes, on its own, is not enough and that any account of qualitative sense data must take account of the fact that we are *conscious* of them (2000, 36). Consciousness in the epistemological sense of availability to thought is tightly linked to the propositional attitude. That is attitudes involving propositions such as "*believing that snow is white, feeling certain that the cat is on the mat,* and so on" (1995, 444). What follows from all of that is that any theory that attempts to reduce sense data to physics and brain chemistry, such in the case of 2, must also reduce the propositional attitude, and there are at least two problems with that.

The first problem is that no theory on offer today is able to reduce the propositional attitude. This leads us to conclude that either we were mistaken to think that qualitative identification has the problem of the reducing theory solved, for we have no idea what a theory that reduces the propositional attitude looks like, or, that any reducing theory will include a substantive part of our propositional attitude. Again, it is not enough to insist that some such theory exists, for recall identification is supposed to be an empirical question. Also, opting for the latter option fares no better. That is because the propositional attitude, it has been argued, *cannot* be reduced to physics and brain chemistry for the following reasons.

First, we have the well-rehearsed argument for semantic externalism, according to which, generally, the set of all facts that determines the content of our propositional attitude include factors that are external to the organism. This is defended in Putnam (1975) where

we put forward his now famous twin earth thought experiment – we will say more about Putnam's semantic externalism in chapter three. Second, we have the argument from "multiple realizability", according to which a mental kind can be realized by many distinct physical kinds (2006). Both of these considerations lead Putnam to conclude that at least in the current context, qualitative identification, and, consequently, theoretical identification is an empty project. This brings us to the final candidate for the meaning of identity, and we shall look at it below.

1.3.1.2.1.3. Identity as Anomalous Token Identity

The view termed "anomalous token identity" is due to Donald Davidson (2001) who, explains Putnam, while acknowledging that there is not *type* identity of the kind we may find in other sciences – earlier on we gave the example of light being electromagnetic radiation, between psychological and physical description, holds, nonetheless, that there is *token* identity. If we assume an ontology of "events", which was Davidson's ontology of choice, then we can state token identity as follows.

Each token mental event is identical with a token physical event. In more concrete terms, each particular psychological event such as someone's thinking about something, or someone experiencing something is identical with a physical event such as perhaps the firing of a group of neurons in the brain (36). Davidson provides the following criterion for identity. He writes "events are identical if and only if they have exactly the same causes and effects" (Davidson 2001, 150). Put more formally, given that x and y are events

(x = y if and only if ((z) (z caused x ↔ z caused y) and (z) (x caused z ↔ y caused z)).

However, Putnam remarks, following Quine, that Davidson's criterion is viciously circular, particularly because of the problem of providing criteria for *individuating* events. To highlight that, Quine provides a simple criterion of individuation (Quine 1985, 166).

$$x = y \leftrightarrow \forall z\ (x \in z \leftrightarrow y \in z)$$

This criterion, Quine remarks, defines identity, whether for events or others, but simply fails to individuate them. The reason for that is that quantifying over a class z, Quine explains, makes sense only when talk of *class* z makes sense, and that would require that classes be individuated. He adds that it is of no help here to invoke of the law of extensionality to individuate classes, for the latter only works if the members of the class are individuated, which is the crux of the matter (166). An illustrative example is provided as follows.

Suppose, we are told, that we want to decide whether the firing of a small group of neurons is token-identical with a particular experience of blue. Such firing, Putnam explains, will have many effects not all of which we would usually associate with that experience of blue. One such effect is the firing of other neurons as well. We already said that the firing of these

neurons is part of the *effect*, but what made us say that? Admittedly, because we had already decided that the event we are investigating is that of the firing of *that* small group of neurons in particular. However, had we decided to include a larger part of the brain, the firing of the latter group of neurons would have been part of the event itself and not an effect of it. The problem is that we do not know how to decide which of the two events is indeed identical with that particular "experience of blue". Davidson's account may not be viciously circular, in the sense that "identity" does not figure on both side of the bidirectional, but it ends up being useless, when we are left with a "criterionless and sui generis sort of "identity"" (2000, 37).

We noted in the beginning that direct realism faced challenges of both epistemological as well as metaphysical kinds. What we have been so far concerned with were Putnam's responses to the epistemological challenges. In what follows we look at the metaphysical challenges as well as Putnam's responses.

1.4. The Metaphysical Challenge to Direct Realism

The metaphysical challenge centres on a distinction between "primary qualities", such as shape, size, etc., which belong to things in themselves and "secondary qualities" such as colour, texture, etc., which belong not to things in themselves but to the mind of the perceiver (2000, 38).

As Peter Ross explains (2015), this distinction originated in the seventeenth century particularly in the works of Galileo, Hobbes and Descartes, and was later developed by Boyle and Locke. It was the philosophical offspring of wedding the, then fashionable, mechanical philosophy with the theory of mental representation. Accordingly, the division was between those qualities that were explanatorily pertinent to the physics of the time which were thus deemed 'fundamental', and those that were not. As science progressed, however, and Newtonian mechanics no longer enjoyed the status it once had, the distinction persisted, yet it was thoroughly reworked so as to accommodate the science of the time.

As such, today, the distinction is based less on the 'fundamental' explanatory role of primary qualities and more on the metaphysical perceiver-dependence of secondary qualities. This dependence is metaphysical because, as will be clear, in truth, for the proponents of the distinction, secondary qualities do not really belong to the real world, but merely appear as such. Putnam provides three responses against this distinction.

Putnam's first response mentions Bishop Berkeley arguments to the effect that whatever argument that attempts to relegate secondary qualities from external world to the mind, if true, applies equally to primary qualities. Putnam does not specify

which argument he has in mind, but we can find two arguments in Berkeley that fit the bill:

1. The argument from the inseparability of primary and secondary qualities.
2. The arguments from the relativity of secondary qualities[2].

1.4.1. Argument from Inseparability

Granting the distinction between primary and secondary qualities with the latter belonging, as we said earlier, to the mind, the first argument can be roughly reconstructed as follows:

1. If primary qualities cannot be abstracted from secondary qualities, then primary qualities cannot exist apart from secondary qualities.
2. Primary qualities cannot be abstracted from secondary qualities.
3. Secondary qualities exist in the mind

Primary qualities also exists in the mind

Of course, this, for Berkeley, is supposed to be an argument for idealism. But in order for it to go through one must accept 3, which Putnam does not. However, if we, at least for the moment, suspend judgment about 3 while accepting 1 and 2, what we arrive at is the conclusion that primary qualities and secondary qualities exists in the same domain, so to speak, meaning that it is the case that either both belong to the mind or that both belong to the world. Hence, those that accept that primary qualities belong to the world must also accept that secondary qualities also belong to the world.

1.4.1.1. Secondary Properties as Dispositions

Some proponents of the primary qualities/secondary qualities distinction may retort that Berkeley's argument, put in this way, is too simplistic. They may remark that to ask whether secondary qualities reside in the mind or in the world would be to commit the mistake of treating secondary qualities as occurent, non-dispositional properties. These more sophisticated proponents may accept that there is nothing like the occurent secondary qualities we think we experience *out there*, yet hold that this does not mean that they exist in the mind either. For secondary qualities, they hold, are, in fact, dispositional properties. And this admits of an 'objectivist' as well as a 'subjectivist' reading, both of which Putnam takes on, in turn.

1.4.1.1.1. Objective Dispositions

[2] These are borrowed from Dicker (2011).

Putnam invites us to consider “color” which is considered an exemplar of secondary qualities. On the objectivist reading, to make sense of color on dispositonalist ground, we need to introduce the notion of *reflectancy*. Color, then, we are told, is a function of reflectancy, which Putnam explains, is “the disposition of an object (or of the surface of an object) to selectively absorb certain wavelengths of incident light and reflect others” (1987, 5). However, this does not take us very far. That is because reflectancy itself does not have a uniform physical explanation. That is because, the color red, for instance, is multiply-realized by different physical means in a variety of red objects. This leads Putnam as far as to claim that “there may well be an infinite number of different physical conditions which could result in the disposition to reflect (or emit) red light and absorb light of other wavelengths (5). As such, according to Putnam, the purported underlying non-dispositional base is too heterogeneous to explanation the dispositional property of colour, and the objectivist reading is unsatisfactory.

1.4.1.1.2. Subjective Dispositions

This gives way to the subjectivist reading according to which, there, in fact, is no such physical magnitude as color but only a disposition in the object itself to *cause* in me a simple, uniform non-dispositional ‘idea’, ‘impression’, a ‘sense datum’ (1987, 6). Here again we see recourse to the notion of sense data, which, Putnam earlier remarked, is too ambiguous to do explanatory work as the notion itself is in need of explanation.

Nonetheless, if we, for the sake of argument, grant that the notion of sense data does make sense, and thus also grant that secondary qualities are dispositions in the object to cause sense data in the subject, even then, the distinction between primary and secondary qualities is not yet off the hook for reasons related to the very notion of disposition.

Putnam distinguishes between two kinds of dispositions (9):

1. Strict disposition.
2. Ceteris paribus disposition.

A strict disposition, he explains, is a disposition of something to do something ‘no matter what’. This involves physical necessity, which Putnam grants, and gives as an example that no massive object can travel faster than the speed of light. A ceteris paribus position is a disposition of something to do something under ‘normal conditions’ or ‘all things being equal’.

An example of ceteris paribus disposition is the disposition of sugar to dissolve in water. Putnam gives a number of examples to show why such disposition should not be considered strict. These include cases where the solution is already saturated with

relevantly similar chemicals, cases where flash-freezing occurs before sugar dissolves, etc. These examples are reminiscent of the objections raised against the analysis of dispositional discourse in terms of counterfactuals by the use of finks, reverse-finks, etc. He concludes that "there is no reason to think that all the various abnormal conditions […] under which sugar would not dissolve if placed in water could be summed up in a closed formula in the language of fundamental physics" (1987, 11).

But if that is the case and if solubility cannot be rendered by a formula pertaining to the language of fundamental physics, then it is automatically 'demoted' just like color and solidity to the realm of secondary qualities. Importantly, Putnam's objection goes well beyond solubility, and applies equally to all ceteris paribus dispositions. Hence, if the argument is cogent then no ceteris paribus dispositional property belongs to 'things in themselves', not even those belonging to the domain of a science.

This is already a troubling conclusion which may lead many to reconsider the distinction. However, not everyone will be troubled by it. In fact, some may hold that if accepting the primary/secondary quality distinction implies that ceteris paribus dispositions do not belong to things in themselves then so much the worse for them. Conversely, they would be content to hold that ceteris paribus dispositions are something we project onto things in themselves. This, however, far from solves the problem according to Putnam who takes issue with the very notion of 'projection' involved in this maneuver.

According to Putnam a projection is:

> thinking of something as having properties it does not have, but that we can imagine (perhaps because something else we are acquainted with really does have them), without being conscious that this is what we are doing. It is thus a species of *thought*--thought about something (11-12).

The notion of projection assumes *thought about something* i.e. it assumes intentionality. We see thus that accepting that ceteris paribus dispositions are projections leads straight into a form of idealism, where by idealism it is meant the dependence of ceteris paribus dispositions, including battery of common sense features of the world, on thought.

But even this unhappy solution is unavailable to the proponent of the primary/secondary qualities distinction. That is because as discussed in previous section, Putnam does not think that intentionality can be reduced to physics. And if intentionality is understood as a disposition of the mind to be about something, then it certainly is not a strict disposition involving physical necessity, but a ceteris paribus disposition. Yet recall that the very notion of projection was introduced to makes sense of ceteris paribus dispositions and it turns out

that it also depends on it leading to a vicious circle (15). As such the subjectivist reading of secondary properties as dispositions also fail.

1.4.2. Argument from Perceptual Relativity

The second argument against primary/secondary properties divide involves the idea of perceptual relativity. Berkeley considers Locke's claim that warmth and coldness do not reside in objects themselves because the same object can appear warm in one hand hot in the other. He remarks that such argument equally applies to figure and extension, that is because, "to the same eye at different stations, or eyes of a different texture at the same station, [figure and extension] appear various" (1999).

Berkeley's argument has been later reiterated by Bertrand Russell in his *Problems of Philosophy* (2001), however for different ends. In his analysis of perception, Russell introduces the distinction between *appearance* and *reality*, with the former being what things appear to be and the latter being how they really are (2). Russell's example of choice is a table. He explains that although a table *appears* differently to us from different angles and in different lights, we do not really think that the table itself is changing. This observation, Russell remarks, as did Berkeley before him, cuts equally against colour as well as shape. Yet, Russell, unlike Berkeley, does not sink into idealism, for he holds that the 'real' table is none other than the 'physical objet' (4). He concludes that what we can know about this physical object is not its 'nature' but its 'relational structure' (16). Putnam notes at least one hole in Russell's argument.

Russell makes the distinction between the *act* or state of awareness which is mental and its *object* which is not (22). He concludes from the fact that we perceive different properties under different condition that the object of our perception is not the object itself but our awareness of it. Evidently, this conclusion does not follow from this premise alone. It must be further supplemented by another premise, namely that it cannot be the case that we perceive different properties under different conditions if we are perceiving the object itself. In the case of colour, for instance, the premise states that it cannot be the case that we perceive different looks of colours under different conditions if we are perceiving the object itself. But what evidence do we have for this?

According to Putnam the aforementioned claim follows only from a crude notion of colour according to which an object exhibits only a single *definite* colour under 'normal conditions', which implies that whenever we do not see one definite colour under normal conditions, then we are not perceiving the object itself. Yet, he remarks, that the very idea of 'normal conditions' is itself problematic. What makes a particular set of conditions *normal*?

1.4.2.1. The Problem with Normal Conditions

Putnam gives the example of looking at his house in direct sunlight and looking at it in the shades, in each case the colour of the house *looks* different, yet neither condition is 'abnormal'. If we say that normal conditions only involve direct sunlight, then it follows that on any occasion when we are not out in the open, such as, for instance, when we are inside buildings, which is most of the time, then we are not viewing things under normal conditions (2000, 40). This, to Putnam, sounds arbitrary.

But if we do give up on the notion of normal conditions, or at least strip away its epistemic weight, what do we infer from the fact that the same house looks different under different conditions? To this, Putnam empathically responds: "Just that!" That is that a particular colour does have *different looks* under different conditions, none of which, however, is abnormal. Nothing in what Putnam says challenges our intuition that objects of a particular colour have a definite colour. But it challenges the unfounded claim that an object of a particular colour must have *one look* under 'normal conditions'. A red apple, for instance, might look different when seen in the sun, in the shades, under candle or LED lights, etc. it does not, however, cease to be a *red* apple. It will certainly have different looks but none of these, according to Putnam is abnormal.

On that account, colour could be understood as "the potentiality of having different looks under different condition". Looks, on Putnam's account, are relational properties, but that does not make them mental, and we certainly do not have to go as far as Russell's division between appearance and reality to account for them.

So far I have been considering what I called the metaphysical challenge against direct realism. I have noted that this challenge puts a lot of weight on the primary/secondary qualities distinction. I clarified what the distinction is supposed to capture, and followed that up with Putnam's reconstruction and update of Berkeley's arguments against it. These arguments show that primary/secondary qualities distinction cannot carry the metaphysical weight which is ascribed to it and as such constitutes no challenge for direct realism.

Importantly, if we take seriously Ross aforementioned claims regarding the origin of primary/secondary qualities distinction, we realize that Cartesianism cum materialism, to use Putnam's expression, is already assumed in order to generate the distinction, which is presumably meant to undermine direct realism. As such, proponents of primary/secondary distinction are already begging the question on direct realism.

1.5. Direct Realism and Representational Realism

This chapter focused on Putnam's defence of what I called the genus direct realism against its adversaries. I said that in the process Putnam takes special care to attack sense data theories both in their modern and more contemporary form. Putnam here is using two different strategies to defend direct realism. The first strategy, in line with the current philosophical literature, takes direct realism as the default, what I called in section 1.1. our native epistemic position. Accordingly, defence of direct realism consists largely of blocking arguments raised against it (Brown 2008, for a recent defence along those lines see Le Morvan 2004). That is by fending off the epistemological and metaphysical challenges raised against it, direct realism becomes in the clear. But Putnam's second strategy is to deny that indirect realism provides a better account of perception than its direct counterpart and this argument is spread throughout his discussion of the epistemological challenges against direct realism in section 1.3. It is helpful to consider how Putnam's direct realism compares to some of the more recent indirect realist views in the literature.

Direct realism is currently on the resurgence (Genone 2016). Its appeal comes from the fact that attending to our perceptual experiences seems to involve attending to mind-independent objects and features. That's why P. F. Strawson rightly remarks that any theory of perception should account for the fact that "mature sensible experience (in general) presents itself as, in Kantian phrase, an *immediate* consciousness of the existence of things outside us" (1979, 47). The property in virtue of which some mind-independent objects and features are constitutive of experience is often called the 'naïve realist property'. And it is widely accepted that visual experience *seems* to have it. Direct or Naïve realists, as echoed in my discussion of Putnam, hold that some experiences, particularly veridical ones, *do* have it (Nudds 2009). But direct realism is not the only game in town. Indeed, it is not even the most popular game in town. That is because orthodoxy belongs to 'representational realism' or 'representationalism' (Locatelli, & Wilson 2017).

The gist of representational realism is that "the perception of distal physical objects is mediated by both the existence and the 'apprehension' of an internal state of affairs—a representation of the objects" (Robert 2018, 16). There is disagreement within the representational realist camp on the precise nature and structure of the content of experience (see Siegel 2016). For example, is the content of experience conceptual or pre-conceptual or non-conceptual (Kelly 2001, Bermúdez 2009)? Nonetheless, and despite their disagreements, it is generally accepted amongst representationalists that the content of representation has *veridicality, accuracy* or *correctness conditions* (Siegel 2010). These notions, unlike those of truth and falsity, which are generally accepted as bivalent, admit of degrees.

It is important to note, however, that insisting on veridicality conditions is generally thought to be necessary but not sufficient to label an account 'representationalist'. That is, an account that has veridicality conditions is not necessarily representationalist. For example, it is possible to have an experience that has veridicality conditions which are describable from

a third person perspective but whose content is entirely 'subpersonal', i.e. are inaccessible to the first person. Such content would not qualify as content of any conscious experiential state and as such does not qualify as content of perceptual experience proper.

I have noted that the appeal of direct realism is that it seems to account very well for the phenomenal character of perceptual experience. To the question of why our perceptual experience seems to involve a relation to mind-independent objects and features, the direct realist answers because it *does*. But whence the appeal of representationalism?

The primary appeal of the representationalism comes from the advent of cognitive science which treats the mind as a reprensentational device. On this view "all mental facts are representational facts" (Dretske 1995, xiv). And accordingly, all mental processes consist of formation and transformation of mental representations (Jacob & Jeannerod 2003). According to its proponents, representationalism promises to offer a more integrated account of the mind in terms of representational states and processes. It also provides a more parsimonious and unifying explanation of veridical and non-veridical experiences (illusions and hallucinations) under a single explanans, i.e. representational content (Byrne 2009). Illusions and hallucinations, representationalists claim, have the same content as veridical perceptions but they fail to meet their veridicality conditions and as such they *mis*represent.

Conversely, direct realists in general hold, against representationalists, that illusions and hallucinations are not the main explananda of a theory of perception. Instead, they hold that a theory of perception should be focused on the veridical experience. This sentiment is shared by Putnam, who, as I explained in section 1.3., does not feel threatened by even hypothetical cases where non-veridical experiences seem qualitatively indiscernible from ordinary perception. For these, for Putnam, are categorically different, even if they appear qualitatively same.

It is important to note at this point that there are different ways of making the direct/indirect realist distinction in the literature, and although it seems prima facie the case that representationalism is incompatible with direct realism, many representationalist hold otherwise (Locatelli, & Wilson 2017). These, unlike data sense data theorists who take sense data to be the *objects* of perception, hold that the representational contents of experience are not themselves objects of perception but only *means* by which we perceive objects. This opens the door for more hybrid views such as Heather Logue's weak content (Logue 2014) and Susan Schellenberg's mutual dependence views (Schellenberg 2014), both of whom, at bottom, hold that veridical experience fundamentally consists of *both* the subject's perceptually representing her environment as being a certain way and as being perceptually related to entities in her environment.

Prima facie it may seem that Putnam would rule out any form of representationalism since such an account posits an intermediary between person and mind-independent object and

properties in the form of representational content. But I think Putnam, who's an avowed naturalist, would be keen that his account of direct realism be informed by developments in cognitive science – in section 2.4. I highlight a disagreement between Putnam and McDowell on this very point. I believe that a hybrid view along the ones suggested above would be welcomed by Putnam. Particularly, Putnam, can echo Bill Brewer's (2017) response to recent attack on direct realism, by arguing that representational content and its ilk may be 'essential psychological enabling conditions' of our perceptual relations to external objects, yet they are not themselves objects of perceptual awareness, since the latter can only be mind-independent external objects and their features.

Before I wrap up this chapter, I want to address an issue that may arise since my thesis is about scientific realism. It may be asked "how do considerations of direct realism in perception advance the realism debate construed narrowly as the debate over the existence of and our commitment to observables"? The answer to that is that direct realism does not *advance* the debate over unobservables, but it makes the debate *possible*. That is because Putnam shows, with his MTA – which will be further discussed in section 3.3.1., that without accepting direct realism in perception there is no guarantee that our words have content. This problem cannot be dismissed as one of external world scepticism, which, the claim goes, can be bracketed while dealing with the question of scientific realism for we can readily accept that there is an external world yet conclude under the weight of MTA that we cannot meaningfully talk about it. Direct realism's relevance for the realism debate is that it helps to secure semantic and cognitive footing in reality, thereby making the debate about what the mind-independent world is like possible. This includes, amongst other things, the debate over unobservables.

1.6. Conclusion

This Chapter presented Putnam's defence of direct realism against epistemological and metaphysical challenges raised by modern and more contemporary philosophers. Such attacks have mostly come from proponents of some form of sense data theory of perception, who present their views as being a more scientifically respectable alternative to direct realism because the latter, they claim, faces severe problems. Conversely, Putnam's discussion shows four things:

1. The challenges raised against direct realism are not as severe as proponents of indirect realism claim, and they can, in fact, be easily solved if we adopt the right understanding.
2. Sense data theories are no more scientifically respectable than direct realism.
3. Sense data, at best, provide an alternative jargon to describe certain events which can be described without them.
4. Sense data bring with them a host of sceptical problems which have plagued philosophy ever since the seventeenth century.

It may appear that such a conclusion gives us strong reasons to accept direct realism, but it does not entirely refute indirect realism or what Putnam calls Cartesianism cum materialism. But here we should keep in mind that Putnam is not attempting such a refutation. For we said earlier on that Putnam's starting point is not agnosticism about the proper framework to deal with the question of perception. Our native epistemic position, i.e. the natural realism of the common man, is a form of direct realism. What the foregoing discussion shows is that the sceptic gave us no good reasons to doubt it, let alone to give it up. In the next chapter we look at the particular species of direct realism that the later Putnam defended.

Chapter 2: Putnam's Account of Direct Realism

2.1. Introduction

The previous chapter served to explain Putnam's defence of the genus direct realism which in the process attacked sense data theory understood broadly to include the immaterialist reading of early moderns as well as the materialist reading of some contemporary philosophers. This chapter serves to explains Putnam's own species of direct realism which he developed by engaging with McDowell's own views on perception.

In section 2.2., I begin my explanation by putting forward McDowell's views on perception as construed by Putnam. This is cashed out in terms of four theses: 1) Openness, 2) Perceptual Disjunctivism, 3) Minimal Empiricism, and 4) Perceptual Conceptualism. Having these laid out at early on will serve as an anchor for my discussion of Putnam's species of perception as Putnam arrives at his own position by taking a stand on each of these theses. I note that what Putnam's McDowell calls 'openness' is none other than what I called the genus direct realism which, as I described in the previous chapter, is the general framework which treats perception as direct – 'openness' meaning open to the mind-independent world. I will not spend more time discussing this thesis because both Putnam and McDowell accept it and I have already said much about in the previous chapter.

In section 2.3., I discuss Putnam's view on disjunctivism –the position according to which in veridical perception there is no such thing as sense data. The later Putnam, under the influence of McDowell, started off as a disjunctivist – as is clear by his continuous attack on sense data we saw in the previous chapter, but he later came to reject disjunctivism under the influence of Ned Block. Hence, Putnam ended up accepting the existence of sense data, but as I will explain he does not ascribe to them the essential role they are generally thought to have in perception by indirect realists.

In section 2.4., I discuss Putnam's view on perceptual conceptualism – the position that all sensations presuppose our conceptual powers. Drawing on results from experimental psychology, particularly the phenomenon known as 'phenomenal richness', Putnam denies perceptual conceptualism. His denial amounts to the modest claim that not all sensations presuppose our conceptual powers. I do not devote a particular section to discuss Putnam's take on 'minimal empiricism' – the position that sense impression is what ultimately justifies perceptual beliefs. Putnam accepts with McDowell that whatever justifies perceptual beliefs must be conceptual. Yet from that and the fact that he, against McDowell, denies perceptual conceptualism, it follows that he also rejects minimal empiricism without having to directly engage with the thesis. In section 2.5., I introduce a key component that Putnam thinks is often missing in the discussion of perception, namely 'apperception'. Putnam holds that apperception occupies a niche between mere sensation and perceptual beliefs. For Putnam, apperceptions are conceptual unlike sensations so they can serve to justify

perceptual beliefs but they are unlike perceptual beliefs in that they are not explicit judgments so they help to explain how something may seem to us to be the case even when we know that it is not.

In section 2.6., I summarize Putnam's species of direct realism. It says that we experience mind-independent things. It accepts that we may have sense data in veridical experience, but denies that sense data are the basis of empirical judgement. It also denies that sensations provide justifications for perceptual beliefs. It finally accepts apperceptions as a key component of perception and an intermediary between sensations and perceptual beliefs.

2.2. Putnam's McDowell on Perception

According to Putnam (2016), McDowell's view on perception comprises four theses. These are:

1. Openness Thesis: "our [sensory] impressions are not *representations*, *we are directly aware of the goings-on themselves*" (189).
2. Perceptual Disjunctivism: "in veridical perception— say, seeing objects in one's vicinity as they actually are— there are no such objects as sense data at all" (155).
3. Perceptual Conceptualism: "*all* experiences, indeed all sensations, involve and presuppose our conceptual powers" (144).
4. Minimal Empiricism: "sensory impressions must be a "tribunal" before which our beliefs about the world can stand" (183).

According to Putnam (2016), McDowell is a direct realist with an epistemic worry. He is not satisfied with a reliabilist account of justification and other epistemic notions because such an account, if understood in a reductionist fashion, cheats us of intentional notions such as reference, truth, justification etc. In the context of the philosophy of perception, McDowell can be understood as seeking to develop an account of direct realism according to which perceptual experiences do not merely causally 'trigger' in us true beliefs more often than not, but instead they *justify* beliefs about the world. That perceptual experience plays a justificatory role in our acceptance or rejection of perceptual beliefs McDowell calls "minimal empiricism" (141).

Yet an eminent sceptical challenge threatens to undermine even this minimal empiricism. This is what James Conant (2012) calls 'Kantian skepticism'. Unlike 'Cartesian skepticism' which Conant uses to denote the skepticism concerning the possibility of knowledge of things outside the mind (8), "Kantian skepticism" denotes skepticism about the possibility of our thoughts having semantic content irrespective of whether these thought are about the mind or the external world. In other words, it concerns how "marks and noises" we make in a linguistic context come to acquire *meaning* (16). This kind of skepticism, Putnam remarks, is not epistemological (2016, 186). In fact, it is more fundamental, for it threatens to

undercut even Cartesian skepticism because if our thoughts do not have content then the very challenge posed by the Cartesian skeptic becomes meaningless (142).

According to Putnam, what imparted on Kantian skepticism a sense of urgency for McDowell is the latter's interpretation of Wittgenstein's Private Language Argument understood in the context of Wittgenstein's remarks on Russell's claims that basic concepts acquire meaning through ostensive definition. That is through "acquaintance by introspection" which takes places through a form of 'pointing to inner impressions'. The problem with this approach is that concepts are *general*. So in order to get at a concept of say, a colour red, given that I am unable to direct my attention to *all* red sense data – which includes future ones, then an abstraction is required. This would, in turn, take place through an internal ostensive definition. Yet, all ostensive definitions presuppose an, at least implicit, understanding of some relevant sortal concepts whose meanings, again, depend on other internal ostensive definitions involving other relevant sortal concepts and we soon find ourselves led into a vicious regress. By reductio McDowell concludes that:

1. Not *all* concepts acquire their meaning through ostensive definition.
2. "Bare presences can't explain how language and minds "hook on to the world"" (142-143).

'Bare presence' here is understood as the non-conceptual deliverance of sensibility or simply 'content' for those who, if we use Davidson's terminology, take experience to be a combination of (conceptual) scheme and content. This McDowell rejects. According to McDowell, to claim that bare presence, passively received, can do any justificatory work is to harken back to the 'myth of the given' which says, introduced by Sellars (1996, 39) can be summarized as the claim that

> some of our terms or concepts, independently of their occurrence in formal and material inferences, derive their meaning directly from confrontation with a particular (kind of) object or experience (deVries, 2011)

Given that McDowell's accepts that the given is really a myth, what solution does he offer to this seeming impasse?

Putnam explains that one solution suggested by Davidson, was to deny that justification begins with experience, and to hold instead that justification begins with *beliefs*. Yet this does not solve the issue, for how can something that is unjustified be able to justify anything? A way out, as is well known, lies in accepting a coherentist theory of justification. But to accept that, for McDowell is to relinquish minimal empiricism, which, for him, is not acceptable.

Seeing as neither receding to the myth of the given nor jumping on the bandwagon of coherentism proves tenable, Putnam explains, McDowell's solution is to deny that

experience is *merely* the product of our sense organs, i.e. that experience is devoid of any conceptual content. Instead, he argues that our conceptual capacities are always active in experience. This means that both our beliefs as well as our experiences are conceptually articulated and, as such, can, instead of being mere triggers of beliefs, play a role in rationally constraining them (2016, 144). In short, the solution for McDowell was to accept 'perceptual conceptualism'. Summarizing McDowell's views, in preparation to consider Putnam's, I say that McDowell is a direct realist, particularly a disjunctivist, who accepts minimal empiricism, but fearing Kantian skepticism, eventually had to supplement it with perceptual conceptualism. In what follows I consider what Putnam thinks about each of these, starting with disjunctivism.

2.3. Disjunctivism and the Place of Qualia in Direct Realism

We noted earlier on that a hallmark of Putnam's 'conversion' from internal realism to common sense realism is his acceptance of direct realism which he coupled, at least earlier on, with perceptual disjunctivism. Although these are two distinct theses, Putnam, under the influence of Austin's *Sense and Sensibilia* (1962) and McDowell's *Mind and World* (1996), accepted the latter because he was keen to deny one dogma of empiricism, namely that perception begins with sense data. His reasoning behind that was pretty straightforward. That is accept disjunctivism i.e. accept that in veridical experience there are no such objects as sense data, and from that it follows that it cannot be the case that perception begins with sense data (157).

Later, however, Putnam came to reject this very argument holding that the work of Ned Block shows precisely that "there is a qualitative and nonconceptual dimension of experience that can be scientifically investigated" (156). This is none other than sense data or to use the term that Block revived *qualia* – following Putnam we will use these two notions interchangeably. Putnam now holds that "there are *qualia,* and progress can be made and *has* been made in saying something about their neurophysiological basis" (156). Qualia are thus back on the scene. But if they do not provide the basis for empirical judgement, a question emerges, namely what becomes their place in experience?

In answering this question, Putnam begins by making the innocuous observation that the mere fact that our experience has a qualitative - nonconceptual - dimension does not entail that what we perceive is qualia (157). We already noted that colours, for instance, are for him, potentiality of having different looks under different condition where looks of a particular colour are irreducible relational properties. Qualia in his framework have a different role. They denote "nonconceptual phenomenal characters common to both veridical and nonveridical experiences" (182). To illustrate, Putnam invites us to consider seeing a blue shirt, there are different things which we can attend to in this experience. These are:

1. The objective colour of the shirt, which is *blue* in this example.
2. The (objective) *looks* that this colour potentially has under different conditions.
3. The (subjective) *looks* for a particular person.

In a veridical experience, all of those, we are told, are genuine properties of the object seen (158). Yet it is strange that Putnam would consider the *subjective look for a particular person* as being a property of *the object*. But I think this is a slip of Putnam. Elsewhere, he explains that these three components constitute "the phenomenology of perceptual experience" (2012, 320). So although the subjective looks do not belong to the object, they, nonetheless, belong to the phenomenology of the perceptual experience of the object, and this sounds more plausible. Putnam, it is likely, was led to this slip while trying to (over)emphasize his break with disjunctivism which claims that the phenomenal quality of visual experience is *exhausted* by the objective properties of the environment. Putnam's point, against disjunctivists, is that sense data generally do exist in veridical perceptual experience even if they do not do epistemic work.

Subjective looks understood as nonconceptual phenomenal characters of both veridical and nonveridical experiences, it should be clear, are none other than *sense data* or *qualia*. They should be understood within the context of Block's remarks, endorsed by Putnam, that "phenomenality outruns (full) cognitive access" – this will be explained at length when we discuss Putnam's criticism of perceptual conceptualism. We are aware of qualia. They genuinely belong to the phenomenology of the perceptual experience. But we may not always have cognitive access to them. As such we are not always able to conceptualize them. Yet, when we do attend to them, explains Putnam, "many of them can be conceptualized to some extent both when they occur and after they occur" (164). An example at this point is helpful.

Putnam considers looking at the beach in bright sunlight. He notes that the exact colour of the sand looks different depending on whether he looks at it with his right eye shut, or with his left eye shut. Each gives an experience of a different shade of yellow. The difference between these two *looks,* however, is not so extreme so as to affect his matching performance on a colour chart. The sand in both cases is yellow. But such looks are not objective, i.e. they do not belong to the object itself. But they do belong to the *experience* of the object, and oftentimes when we do attend to them we can conceptualize, and liberally talk about them. What's more is that, in the case of vision at least, we know why we have these looks. That is due to difference in pigmentations of the macule of each eye. And this difference, we should add, is normal.

It is also noteworthy that not all experiences are accompanied by qualia. Putnam gives the example of "raising one's arm intentionally" – as opposed to it simply 'going up'. He notes that such an experience is not accompanied by a 'quale of voluntariness' (2016, 164). This example helps as a further elucidation of the place of qualia in Putnam's framework. Not

only are they not the basis of experience, they sometimes are entirely absent from experience. But if qualia do not play the epistemic role that traditional empiricist want of them, does that mean that they do not have *any* epistemic role to play?

According to Putnam, to jettison bare qualia as basis of empirical judgment is not to strip them from *all* epistemic roles. They can still have an epistemic role, only now it is more modest than the one they previously played. We just noted that we are sometimes able to attend to our own qualia just as we are able to attend to properties of external objects. We do that when we are less interested in the worldly object experienced and more interested in the "what is it like" to experience such an object. In such cases, qualia can become objects of experience just as any ordinary object such as trees and rabbits (195).

Notice that on Putnam's account, qualia are no longer *the* object of experience. Instead they are, or can be, *an* object of experience. Putnam's resituation of qualia shows, against McDowell, that we need not to go as far as to accept disjunctivism in order to secure direct realism. For the latter can easily accommodate qualia given proper understanding. We now consider Putnam's take on 3 which as we noted earlier, is inextricable from 4 – but we will further clarify it below.

We have previously explained why, Putnam thinks, McDowell opted for 4. Importantly, Putnam remarks that despite McDowell's Kantian bent the former remains heavily influenced by the empiricist's position. This, Putnam notes, is clear in McDowell's writings where he uses "experience" and "impressions" interchangeably, and when he emphatically insist on preserving "minimal *empiricism*" (159). This helps to highlight why, according to Putnam, McDowell is keen on maintaining sensations or impressions as the basis for judgment. Yet as we noted earlier, adopting 4 on its own will not do, for it renders empiricists vulnerable to Kantian skepticism. That, we explained, is why McDowell had to supplement minimal empiricism with perceptual conceptualism. Given that without the latter the former is untenable, Putnam directly attacks 3 and if 3 falls 4 follows.

2.4. Against Perceptual Conceptualism

In a recent paper published jointly with Hilla Jacobson (2016), Putnam criticized 3 both on philosophical and methodological grounds. Jacobson and Putnam explain that McDowell's claim that perceptual experiences are conceptualized through and through is made on *aprioristic* grounds in the form of a transcendental argument which aims to determine the semantic and epistemological role that experience *must* play in order to secure 1, i.e. 'openness' (2).

They note a number of, what they take to be, counterintuitive conclusions that follow from such a view, such as that perceptual awareness in rational and non-rational animals is different in kind, and that, given that concept-use is a linguistic achievement for McDowell, learning a language is a prerequisite for perceptual experience (3). Jacobson and Putnam

disagree with McDowell's methodology which says that the question of whether or not our experiences are conceptualized through and through is answerable a priori as well as his conclusions. Instead they argue that such a question must be settled on empirical grounds and they provide an empirically-grounded argument to the effect that our experiences are, in fact, not thoroughly conceptual. This argument we look at next.

2.4.1. For Phenomenal Richness

Jacobson and Putnam's argument begins by considering what they call the "phenomenal richness hypothesis", which asks whether our perceptual experience is 'rich' or 'sparse'. This can be illustrated in the context of 'change-blindness experiments' where, for instance, a perceiving subject is unable to unreflectively detect noticeable changes in two successively presented images, which, otherwise, she would consider impossible not to notice.

The question of phenomenal richness in cases of change-blindness centres on whether that person actually fails *to see* i.e. lacks phenomenal consciousness of the changing feature – call this a "change-aspect", or whether they do see the change-aspect but *lack cognitive access* to it. If the former case is true we say that experience is phenomenally sparse, otherwise it is rich.

Jacobson and Putnam, following Ned Block (2008) and Victor AF Lamme (2010), hold that recent experiments by Landman et al. (2003), lend support to the phenomenal richness hypothesis of visual experience. Particularly, they accept Block's explanation that the lack of (full) accessibility, despite the change-aspect being represented by the subject is roughly that "the 'capacity' of the visual phenomenal memory system is greater than that of the working memory system that governs reportability" (5). Put more simply, they hold that the data from Landman et al. support that "phenomenality indeed outruns (full) cognitive access" (11). In what follows I consider how we can go from phenomenal richness to perceptual non-conceptualism, keeping in mind the latter notion is meant to capture the modest claim that *some* aspects of our experience are nonconceptual.

2.4.2. From Phenomenal Richness to Perceptual Non-Conceptualism

To begin, I must first establish what is meant by conceptualism. Jacobson and Putnam explain that

> According to the standard understanding of conceptualism, the contents of experiences are conceptual in that they cannot be attributed to their subjects without *ipso facto* attributing to those subjects *mastery* of the concepts required for their specification (11).

By emphasizing mastery, it becomes clear that to argue against perceptual conceptualism, it does not suffice to show that the subject does not *apply* a concept – this, indeed, is

supported by Landman et al.'s experiment, what must be shown is that the subject does not *possess* the concept and this they hold is not obvious from the experiment.

Before we proceed, it is important to highlight that Jacobson and Putnam distinguish between change that can be easily perceptually conceptualized such as the contrast between, say, "up" and "down", "right" and "left" and "horizontal" and "vertical", etc., and changes that are difficult, perhaps even impossible, to perceptually conceptualized. That latter change would have to rely on what is called "demonstrative concepts". McDowell introduced demonstrative concepts to defend conceptualism against cases which, at least prima facie, present us with experiences that are richer and more fine-grained than what is afforded to us by our conceptual repertoire, such a particular shade of green. According to McDowell, "one can give linguistic expression to a concept that is exactly as fine-grained as the experience, by uttering a phrase like "that shade", in which the demonstrative exploits the presence of the sample" (1996, 56-57).

According to Jacobson and Putnam's, there is no reason to think that the phenomena of change-blindness are restricted to aspects which are easily (perceptually) conceptualized. They explain that although Ladndman et al.'s change-blindness experiments relied on easily perceptually recognized concepts such as changes of orientation (of rectangles) from vertical to horizontal, it is highly plausible that these experiments would yield similar results if the change of orientation was at an angle such as "at 35 degree" pre-change and "125 degree" post-change. Such changes, however, would not be easily perceptually conceptualized and would have to rely on demonstrative concepts.

2.4.3. The Standard Conceptualist Thesis

This allows Jacobson and Putnam to arrive at the following claim that they hold must be accepted by any conceptualist:

> *Background Assumption*: Some of the relevant phenomenally conscious aspects are susceptible only to perceptual demonstrative conceptualization (12).

The background assumption is supposed to capture "the indispensability of perceptual demonstrative concepts for the (perceptual) specification of some phenomenally conscious aspects" (13). We noted earlier that on the standard understanding, conceptualism requires that the subject possesses the concept. This is captured by Jacobson and Putnam as follows:

> *The Standard Conceptualist Thesis*: In order to enjoy an experience with phenomenally conscious aspect P, the subject must possess the concept C (the concept needed for the specification of the content of P) upon having the experience (14).

It follows from the standard conceptualist thesis in conjunction with the background assumption that, for some experience with P, C must be a perceptual demonstrative

concept. Also, despite that concepts which rely for their meaning on demonstrations of the form "that X" are not available to the subject prior to the demonstration, they *must*, holds McDowell, be available for future experiences if they are to be concepts at all. That is because concepts belong to thought and for anything to count as thought it is necessary that it be at a "distance from what would determine it to be true" (1996, 57).

But if that is the case then the question becomes what allows a demonstrative perceptual concept such as "that X" to be a *concept*, especially that something of that form is short-lived? McDowell's answer is that what ensures that is a certain "associated recognitional capacity" which carries over to the near future the ability to use the concept in thought while securing the required distance (57). This allows Jacobson and Putnam to arrive at condition that must be met in order to have the relevant concept on the standards conceptualism view:

> *The recognitional requirement for possession of perceptual demonstrative concepts*: A necessary condition for the possession of a perceptual demonstrative concept C at time t, is having the ability to (perceptually) recognize instances falling under that concept at time t+ ε (2016, 14).

Now given that according to the standard conceptualist thesis, in order to enjoy an experience with P one must possess C, and given that on the recognitional requirement condition possessing a perceptual demonstrative concept requires the ability to perceptually recognize instances falling under it at t+ ε, Jacobson and Putnam derive the following:

> *The standard conceptualist requirement for the instantiation of phenomenal aspects*: A necessary condition for having an experience with phenomenal aspect P at t is having the ability to (perceptually) recognize instances falling under concept C at time t+ ε (14-15).

McDowell even remarks that such a recognitional capacity "sets in with the experience (1996, 57). Thus the phenomenal perceptual experience and the recognitional capacity, for him, go together. Yet, remark Jacobson and Putnam, the question of whether enjoying an experience with P requires a capacity to perceptually recognize a concept as falling under C at t+ ε is itself an empirical question, not to be answered a priori. They note that a simple phenomenological reflection is enough to cast doubt on such a thesis. More importantly, they hold that the phenomenon of phenomenal richness which is supported by the experiments Landman et al.'s rule out McDowell's condition, for these experiments show that

> There are P aspects at time t, such that the subject (who lacks the ability to notice subsequent changes in those aspects) lacks the ability to (perceptually) recognize instances falling under the concepts needed for their specification at time t+ ε (15).

I can summarize Jacobson and Putnam's argument as follows:

The standard conceptualist thesis in conjunction with the recognitional requirement and the background assumptions entail that in order for one to enjoy an experience with P at t, the subject must be able to perceptually recognize instances falling under C at t+ ε. However this is contradicted by the findings of the change-blindness experiments which show that phenomenality outruns (full) cognitive access, thus leaving the perceptual conceptualist with an inconsistent triad. To make her way out of this predicament, it seems that the perceptual conceptualist must at least modify the recognitional requirement so that it would not clash with the findings of the change-blindness experiment, although the standard conceptualist thesis can also be weakened as we will see in due course. But for now let us focus on the recognitional requirement.

2.4.4. Weakening the Conceptualist Thesis

We said that the change-blindness experiments shows that subjects at one time have a limited cognitive access. Landman et al.'s experiments particularly show that subjects were only able to recognize a combination of four items out of the entire eight on display. What determines which combination in particular is recognized ultimately depends on the subject's attention. Thus it could be that the conceptualist may be able mitigate that lack of full cognitive access by exploiting attention This is done by relaxing the recognitional requirements into the following counterfactual.

> *The attention counterfactual*: For any phenomenal aspect, had the subject focused his attention on the feature represented *via* that aspect, she could have recognized it at time t+ ε (17).

Yet this counterfactual is not sufficient to salvage perceptual conceptualism, at least in its standard form. That is because it removes the very thing that motivated the recognitional requirement in first place, which the 'anchoring experience' that is supposed to fix the meaning of the concept. For if we consider a certain concept C at t which the subject did not pervious have, if all that is required to have C at t is the mere possibility of recognition in *different* circumstances –which trades on attention, then there is nothing really fixing the meaning of C in the *current* circumstances, nor, to be sure, in different circumstances. The subject would simply lack the anchoring experience. Given that this move is blocked, the other option would be to also weaken the standard conceptualist thesis into something more defensible:

> *The Weak Conceptualist Thesis*: In order to enjoy an experience with phenomenal aspect P, the subject must be able to *acquire* the concept of C (the concept needed for the specification of the content of P) upon having the experience (18).

On the weak conceptualist thesis, the subject is no longer required to *possess* the concept but should only have the *ability to acquire* it. Jacobson and Putnam note that the weakening

of the conceptualist thesis leads to the weakening of the counterfactual requirement which now reads as:

> *The counterfactual requirement for the ability to acquire perceptual demonstrative concepts*: Being able to acquire concept C at time t requires that had the subject focused her attention on the features which fall under C at t, she could have recognized them at t+ ε (18).

From the weak conceptualist thesis and the new counterfactual requirement follows the following requirement for having an experience with a phenomenal aspect P:

> *The weak conceptualist requirement for the instantiation of phenomenal aspects*: A necessary condition for having an experience with phenomenal aspect P at t, is that *had the subject focused her attention on the features represented via that P aspect, she could have recognized them at time* t+ ε (18).

Yet, whether such a requirement must be satisfied in order for a subject to enjoy a phenomenal experience, Jacobson and Putnam remark, is also an empirical question, to which they hold the answer is a negative for at least some phenomenal experiences, namely those that fall outside of attention at t.

To argue that, they begin by noting an observation made by psychologists as early as William James, according to which the intensity of a particular sensation varies with attention. As an example, they note that, on a bookshelf with colourful books, when directing one's attention from one book to the next while keeping one's eyes fixed, one noticed that the shade of colour of the latter appears to slightly change. It becomes more intense.

Jacobson and Putnam do not content themselves with providing phenomenological report however. Instead, they draw support for work in experimental psychology, namely the work of Carrasco et al. (2004), whose experiments show that "covert attention does intensify the sensory impression of a stimulus" (5). And although Carrasco et al.'s experiments focused on apparent patch contrast, Jacobson and Putnam remark that similar experiments shows that such results equally apply to colour saturation, object size, gap size, spatial frequency, etc. with the bottom line being that *attention affect phenomenology* (19). How does this conclusion bear on weak conceptualism?

I noted that weak conceptualism claims that a requirement for having a phenomenal experience is that had the subject focused his attention on the feature represented by the phenomenal aspect, then she would have been able to recognize it at t+ ε. However, for this requirement to be tenable, it must assume that the experience the subject has when she is not attending *looks the same* as in the second experience when she is, in fact, attending. That is, the attention shift, according to the counterfactual, should take place within the *same* phenomenal aspect, and not through some other phenomenal aspect. As such, it is

within the same phenomenal aspect that had subject focused her attention at t, then she would have recognized the feature at t+ ε.

Yet the results of the aforementioned experiments which tell us that attention affects phenomenology falsifies this very presumption when it denies that the phenomenal aspects of experience remain unchanged by attention. Also, it does not help to say that the same feature may be represented through different phenomenal aspects, Jacobson and Putnam tell us, for the latter rely for their conceptualization on perceptually demonstrative concept whose individuation is very fine grained and sensitive to the nuances brought with differences in even very similar aspects. This leads Jacobson and Putnam to conclude that, different phenomenal aspects may or may not exhibit the same features but they definitely imply different demonstrative concepts.

Thus, weak conceptualism is lost either ways. It is lost when we claim that the phenomenal aspect remains the same despite the change in attention, for this, we said, is contradicted by the findings of attention experiments. And it is lost when we relinquish the requirement for the sameness of the phenomenal aspect by accepting that different phenomenal aspect can give us the same feature when attended to, for this yields different demonstrative concepts. Either ways weak conceptualism cannot be maintained. And the reason behind that lies in that the picture furnished by weak conceptualism which assumes that phenomenal aspects are not changed by attention, which itself follows a faulty understanding according to which attention is external to perceptual experience. The upshot as summarized by Jacobson and Putnam is:

> *The lesson taught by experiments concerning the effects of attention on phenomenality*: There are phenomenal aspects at time t – ones that represent features that fall outside the focus of attention – such that had the subject focused her attention on the features they represent, those phenomenal aspects would have changed; thus, *a fortiori* those phenomenal aspects are not such that had the subject focused her attention on the features they represent, he could have recognized those features at t+ ε (21).

Jacobson and Putnam conclude that neither the standard nor the weak version of conceptualism is defensible given the findings of experimental psychology and as such perceptual conceptualism is untenable.

The foregoing discussions reveal why, for Putnam, sense data cannot provide the basis of empirical judgment. But they also reveal why Putnam does not think that McDowell's sense impression can do the job either, thereby rejecting 3. This is because despite major disagreements, Putnam does agree with McDowell on an important point, namely that whatever that is supposed to do the job must be conceptualized, and whereas McDowell thinks that sense impressions fit the bill, Putnam, given that he rejects perceptual conceptualism, think that they cannot. Alternatively what Putnam does think does the job are "apperceptions".

2.5. The Case for Apperception

According to Putnam, when McDowell claims that all sense impressions are conceptualized, he is denying an important distinction that William James makes which has been standard in the psychological literature since at least Kant. That is the distinction between "sensations" and "apperceptions". Putnam explains that attitudes concerning the relations between the two have varied. He tells us that Kant, for instance, held that sensations divorced of apperceptions "are nothing to us", which, he thinks, explains why McDowell holds that there cannot be unconceptualized sensations, whereas, James, for instance, held that line between the two is only sometimes fuzzy (2016, 146).

Yet, we saw in our discussion of perceptual conceptualism that, against both Kant and McDowell, Putnam holds that we can, and, in fact, do have phenomenal experiences of unconceptualized sensations. Hence there is nothing paradoxical about having unconceptualized sensation as well as conceptualized apperceptions. But what exactly is apperception?

In simple terms, apperception, according to Putnam can be understood as the "awareness that something is the case" (2012, 87), or as "the recognition of what one is perceiving" (2016, 147). And although, Putnam does not put forward a fully-fledged account of apperception, he finds plausible the identification of apperception – in the case of vision with what Block calls 'secondary seeing' (148) but we will also see that he does not go all the way so as to endorse it for reasons that will be clear. Block characterizes secondary seeing as a:

> hybrids of visual attributives and concepts applied to objects of primary seeing and complexes of them in states that put together perception with perceptual judgment (2014, 7).

Block, following Tyler Burge (2010), holds that visual attributives are a species of perceptual attributives which, themselves are elements in perceptual content. These along with singular elements constitute every percept or object of perception. Also, primary seeing, here, is understood as

> the application of a visual attributive to a "visual object", i.e. an object that is itself picked out by a demonstrative element in a percept (7).

Thus, the difference between primary and secondary seeing is that the latter involves concepts and is directly involved in the formation of perceptual judgment, whereas the former lacks such features.

On the face of it, Putnam's apperception and Block's characterization of secondary seeing seem to fit pretty well. Yet a closer look reveals that this is not entirely right. For Block was providing an *analysis* of secondary seeing in terms of primary seeing and the ensuing

conceptualization. And although, at once place, Putnam said that he thinks this is plausible (2016, 148), elsewhere, we find him insisting that apperception is a *genuine experience* that is neither the same as sensation nor the same as perceptual beliefs – we will say more about that shortly (2012, 87).

But would apperception still count as genuine experience if it is analysable into primary seeing and conceptualization? I think not. A way around that, for Putnam, is simply to qualify Block's characterization so as to render it not an analysis of apperception but more of providing necessary but not sufficient conditions for apperception. Accordingly, we may say that apperception includes sensation, awareness and conceptualization as necessary component. But it is not *just* the sum of these three.

Putnam, we said, is keen on distinguishing apperception from sensations and perceptual beliefs in order to secure a place for it as a genuine form of experience. He explains that apperception differs from sensations in at least three respects. First, unlike sensation, apperceptions are conceptualized. To apperceive something presupposes a set of requisite background concepts. That is why, according to Putnam, although prelinguistic children and animals may for instance, *sense* a colour or feel pain, they do not *apperceive* the colour as colour or pain as pain, because they lack conceptual abilities (149). Second, unlike sensations, apperceptions can be either accepted or rejected, whereas it makes no sense to ask whether one accepts or reject mere sensations. Third, apperception may not be about something that produces sensations. Putnam calls these "amodal" apperceptions which he characterizes as "awareness of aspects that are "present in absence" such as my awareness of a tomato that I am looking at as having another side" (150). How does apperception compare to perceptual beliefs?

Putnam notes two important similarities between apperception and perceptual beliefs, which are absent in mere sensation, namely, that both require our conceptual backgrounds and that both can be either accepted or rejected. However, he also notes that they differ in at least one crucial respect, namely that apperception, unlike perceptual belief, is not an explicit judgment, although it can, nonetheless, justify judgement (149). This will be made clear with an example.

Consider the Müller-Lyer illusion, which is a visual illusion involving stylized arrows. In this, "a line with inward-pointing arrowheads appears shorter than a line of the same length with reversed or outward-pointing arrowheads" (Colman 2015). Putnam explains that in this illusion, I may have a *seeming apperception* that the line with the inward pointing-arrowheads is shorter than the line with the reverse arrowheads. Still, I do not *believe* what I *seem to apperceive*. The same goes for stick's looking bent in water. I may seem to apperceive that the stick is bent in water, but I do not believe that.

Here, it seems that Putnam finds that apperception on its own is not enough, so he introduces another important distinction between apperception and 'seeming apperception' with only the former counting a genuine success term. With this distinction on board, the difference between apperception and belief becomes more apparent. For whereas one can have a seeming apperception that something is the case, even though he *knows* that is it not (think of the previously illusions), one cannot have a 'seeming belief' that something is the case when something is not (149).

However, there is something ambiguous about 'seeming apperceptions' that Putnam does not clarify, namely what they are. This depends on whether the emphasis is placed on "seeming" or on "apperception" in "seeming apperception".

In the former case, seeming apperceptions would, presumably, be understood as mental states which falsely appear to the subject as, but are not in fact, apperceptions. These obviously are not perceptual beliefs because they do not involve judgement, but they are not bare sensations either because they involve conceptualization. So the question of what exactly these are become rather pressing.

In the latter case, seeming apperception are, in fact, species that fall under the genus apperception. Seeming apperceptions are, on that understanding, contrasted with genuine apperceptions. In this case we do not have to wonder about their nature, for we already know what they are. But the pressing question becomes how can we distinguish between a seeming and a genuine apperception?

I believe that Putnam is caught between the two readings. For both, we saw, have advantages and disadvantages. Yet, it seems that by distinguishing between apperception and seeming apperceptions, as he does, Putnam is pitting the two against each other. Nonetheless, his silence on what seeming apperceptions are supposed to be suggests that perhaps he thinks that it is obvious that they are indeed kinds of apperception. If I had to choose a reading I would go for the latter as it seems a simpler reading which is more in harmony with Putnam's pragmatic, fallibilistic outlook. As such, we would say that apperception is a fallibilistic function – and state, of the mind which can provide justification for perceptual beliefs.

As for how to distinguish between a seeming and genuine apperception, Putnam is not likely to give necessary and sufficient conditions. Instead, given his pragmatism and his apparent hostility to the a priori, which were displayed in his critique of perceptual conceptualism, Putnam is more likely to refer to our *evolving* successful practices for answer. This is supported by his claim that

> But the whole history of science suggests that, when it comes to *non-deductive justification*, the line between "empirical fact" and "method", is blurry: we are often led by the success of novel sorts of theories to reconsider our canons of justification

> themselves. (The whole history of science since the Enlightenment could be described as a sequence of such reconsiderations.) (2012, 90).

2.6. Conclusion

This chapter discussed Putnam's account of direct realism in perception which includes sensations or sense impressions, apperceptions, perceptual beliefs, and qualia. Sense impressions "are a *part* of the causal chain that constitutes the normal formation of a particular perceptual belief on the basis of seeing something in one's visual field" (88), they are not conceptualized and, against McDowell, they are not the 'tribunal' before which all our judgments stand, that is they do not justify judgement. What does that are apperceptions.

Apperceptions occupy a niche between sense impressions and perceptual beliefs. They are conceptualized and they can justify judgement, although they are not themselves judgement. They are noninferential and, unlike perceptual beliefs, they can be "judged wrong even when they *seem* right" (2016, 151). That is why they are the 'tribunal'. I will skip perceptual beliefs, because Putnam does not discuss them, presumably because he has nothing new to say about them. As for qualia, they are the phenomenal nonconceptual aspect of both veridical and nonveridial experience. Against sense data theorists, they have no epistemic role to play in perception save for when they themselves are the object of experience, in cases where subjects are interested in the "what is it like" to experience a particular object.

Chapter 1 and 2 set the stage for the discussion of Putnam views on scientific and metaphysical realism. They are necessary, I said, because according to Putnam it is only through direct realism in perception that we have cognitive access to the mind-independent world. But I also said that direct realism has metaphysical implications, namely that what we perceive, i.e. objects and their features are real. This puts constraints on our descriptions of the world. Pertinent to scientific realism, it raises the important question concerning the relation between the world as directly perceived, i.e the world of common sense and the world as described by our best science, as both . This will be dealt with in what follows.

Chapter 3: Putnam's Scientific Realism

3.1. Introduction

The previous two chapters explored the later Putnam's views on perception. They included a defence of the genus direct realism (chapter 1) and a defence of a particular species of direct realism (chapter 2). These two chapters paved the way for my discussion of scientific realism. They were necessary I said, because, according to Putnam, it is only through direct perception that we have cognitive access to the mind-independent world, and if scientific realism is to be a form of realism about the world then, it must, for Putnam go through perception. I am now ready to explore the later Putnam's views on scientific realism.

In a 1982 paper entitled "Three Kinds of Scientific Realism", Putnam distinguished what he takes to be three forms of scientific realism, only one of which we now consider to properly belong to the scientific realism debate. These are:

1. Scientific Realism as Materialism.
2. Scientific Realism as Convergence.
3. Scientific Realism as Metaphysics.

This chapter highlights what later became pervasive in Putnam's work, namely the intertwinement of the question of scientific realism with that of metaphysical realism, leading us to assert that a proper understanding of Putnam views on scientific realism –in its common philosophy of science usage of the term, is bound be defective without a proper understanding his views on metaphysical realism at that time. Casting aside the materialist sense and focusing instead only on scientific realism as convergence and as metaphysics, the following aims to provide a critical exposition of the Later Putnam's views in the scientific realism debate. This chapter is divided into two parts. The first is on scientific realism as convergence. The second is on scientific realism as metaphysics.

In the first part, in section 3.2., I introduce Putnam's convergent realism characterized in terms of reference, truth and convergence. In section 3.2.1., I explain Putnam's now famous no miracle argument, highlighting that even if cogent, the argument fails to support Putnam's *convergent* realism, as it gives no support to his convergence thesis. This is made worse by Thomas Kuhn and Paul Feyerabend's incommensurability blocking that very thesis, which I introduce in section 3.2.2.. In section 3.2.3., I consider Putnam's responses to the incommensurability challenge in the form of his causal theory of reference and find it wanting.

In the second part, I discuss Putnam's views on scientific realism as metaphysics, what Putnam also calls metaphysical realism. Although Putnam is known to reject metaphysical realism, it will be clear that he only reject one kind of metaphysical realism only to endorse another one. In section 3.3., I explain the metaphysical realism that he rejects, that which is

characterized in terms of ontology, ideology and correspondence. Putnam rejects this form of metaphysical realism under the force of the model-theoretic argument. In section 3.3.1., I briefly explain that argument in turn highlighting the challenge it poses for realism. In section 3.4., I explain the metaphysical realism that Putnam accepts. It is one that accepts existence of a mind-independent world while countenancing two phenomena that are crucial for Putnam, namely 'conceptual relativity' and 'conceptual pluralism'. I also explain these in section 3.4.1. and section 3.4.2. respectively. These discussions will pave the way for a new take on the relation between science and common sense which I take up in section 3.4.3.. I then provide a general assessment of Putnam's scientific realism section 3.5.. In section 3.6., I conclude this chapter with some relevant findings prior to providing a general conclusion to the first three chapters in the section afterwards.

3.2. Scientific Realism as Convergence

Scientific realism understood as convergence, is the one used in the scientific realism debate. Putnam notes that, despite confusion on behalf of his commentators, he always regarded himself as a scientific realist in that sense, even during his internal realist phase – we will explain why that is the case when we consider Putnam's internal realism (2012, 55).

We can characterize Putnam's scientific realism in terms of three theses:

1. *Reference Thesis*: terms in mature scientific theories typically refer.
2. *Truth Thesis*: Accepted theories in mature science are approximately true (1975, 73).
3. *Convergence Thesis*: Subsequent scientific theories tend to converge in the sense that earlier theories are very often limiting cases of later ones, and theoretical terms preserve their reference across theory change (1978, 483).

Notice that the convergence thesis includes two components, one pertaining to *reference continuity,* i.e. terms featuring in subsequent theories refer to the same entity, and *theory convergence*, i.e. later theories are generally more approximately true than earlier ones with the earlier theories acting as limiting case of the later ones under specific conditions. The convergence thesis entails a linear and cumulative account of scientific progress. But as will be clear, convergence thesis, and as a result the linear account of scientific progress, faced important challenges.

3.2.1. The No Miracle Argument and the Pessimistic Meta-Induction

Throughout his career, Putnam continued to support scientific realism with what came to be known as the No Miracle Argument (NMA), which is an ampliative argument, usually made in the form of Inference to the Best Explanation (IBE) that is meant to takes us from the 'success of science' to the approximate truth of scientific theories. NMA for Putnam serves two functions:

i. As an explanation of the success of science and
ii. As an attack on neo-positivists such as operationalists and instrumentalists, for whom, Putnam holds, the success of science amounts to nothing less than a miracle.

NMA has admitted of different formulations, here is one:

(P1) Science is very successful
(P2) the explanation of the success of science is that either
(a) theories are approximately true or
(b) *S* where *S* is any other explanation besides approximate truth.
(P3) approximate truth is the best explanation.

(C) The success of science is due to theories' approximate truth

NMA, I said, is an instance of IBE, which James Ladyman explains, is

> "a (putative) rule of inference according to which, where we have a range of competing hypotheses all of which are empirically adequate to the phenomena in some domain, we should infer the truth of the hypothesis which gives us the best explanation of those phenomena" (2007, 341)

The rationale behind NMA is that the amazing empirical success of science (explanatory and predictive success) cries out for an explanation. And the best explanation of this success is that mature, predictively successful, scientific theories are approximately true. For if these theories fail to approximate the structure of the unobservable reality – structure here is used loosely, then it is extremely unlikely – a miracle – that we would get the empirical success we have.

Antirealists have attacked NMA on the grounds that IBE is not truth-conducive. Bas Van Fraassen, for instance, argued that from the fact that the an explanation is the best we cannot infer that it is (probably) true for we can be dealing with a 'bad lot' of explanations in which the true explanation does not figure (1980, 143). Also, he argued that the criteria we use to assess one explanation as the *best* are not truth-conducive. These criteria are often called 'theoretical virtues' or 'supraempirical virtues' because they are beyond empirical accuracy. They generally are taken to include consistency, scope, simplicity, and fruitfulness (Kuhn 1977, 322). Van Fraassen considers these virtues pragmatic rather than epistemic virtues. Take simplicity, for instance, Van Fraassen argues that the only reason we would accept a simpler explanation as true is because we think the world is simple. But what reason do we have for this claim? Such reasons would be either metaphysical or theological, neither of which, he maintains, are legitimate factors in scientific inferences (1980, 90). Van Fraassen argues that all theoretical virtues that realists often appeal to are like that, i.e. pragmatically useful but not truth-conducive. (But see Schindler 2018 for a recent defence.)

Following Putnam's 1975 pronouncement, NMA was refined and elaborated notably by Richard Boyd and Stathis Psillos, the latter of whom turned it into what came to be known as the 'explanationist defence' of scientific realism. Unlike the usual NMA, his own version, Psillos maintains, "is not a generalization over scientists' abductive inferences". Instead, it is a defence of the reliability of IBE or abduction. In this sense, NMA can be understood as a 'meta-abduction' the explanandum of which is the general feature of scientific methodology. According to Psillos, the best explanation of the reliability of such a methodology is that theories implicated in it are approximately true (1999, 79).

Psillos's updated NMA has also been criticized on several fronts. Like the initial NMA, this updated version is not likely to convince those antirealists who think IBE is not a good form of inference. Also like Putnam's own version, Psillos's NMA is supposed to be a scientific hypothesis/explanation. As such, it should be independently testable. But it is not clear how one can test either version. And if NMA is not testable and as such not a scientific explanation then it is a philosophical explanation. But if a scientific explanation may have testability to speak in its favour it is not clear what a philosophical explanation has in its favour.

One may suggest that we can always appeal to theoretical virtues, the difficulties for which I already mentioned. Nonetheless, the issue here is that these theoretical virtues are generally introduced to break the tie between empirically adequate *scientific* theories. What reasons do we have for supposing that they work for '*philosophical* theories'? Another problem is that it is unclear what the 'scientific methodology' that Psillos has in mind is. It is not even clear that there is such a single 'scientific methodology' that goes beyond rules of thumb which hardly qualify as *the* scientific methodology.

Lastly, Psillos's version of NMA has been criticized on the grounds that it is viciously circular because it is using IBE to validate IBE. As put by Arthur Fine, the explanationist defence of scientific realism is deficient because it involves "the very type of argument whose cogency is the question under discussion" (Fine 1991, 82). Psillos was well aware of that but he tried to salvage his NMA by making a distinction between 'premise circularity' and 'rule circularity', whereby an argument is said to be premise circular if the conclusion is included as of one of its premises. An argument is rule circular, however, Psillos contends, if it employs a rule of inference – but not a premise, that takes us from premises to conclusion. Rule circularity, Psillos holds, is not 'vicious' whereas premise circularity is (1999, 79-87). But as John Worrall pointed out , there seems to be complete equivalence between rule and premise circularity, and any premise circular argument can be turned into a rule circular one by taking out a premise *c* from an argument and adding an extra rule that says you can infer X & c from any derivable statement X (Worrall 2011, 20).

It seems that when it comes to arguing against the antirealist who are IBE sceptics what realists have in NMA is not an argument but an *intuition,* a 'no-miracle intuition' which kicks in when we consider an instance of a striking predictive success. Such an intuition Worrall

argues cannot be turned into an argument without begging the question against the antirealist. But not all antirealists are IBE sceptics. Many antirealists accept IBE, but they along with some realist have questioned NMA due to what came to be known as the Pessimistic Meta-Induction (PMI).

PMI could be traced back at least to Henri Poincare's *Science and Hypothesis* (1905) under the label of the 'bankruptcy of science'. There, Poincare wrote that

> The ephemeral nature of scientific theories takes by surprise the man of the world. Their brief period of prosperity ended, he see them abandoned one after another; he sees ruins pile upon ruins ; he predicts that the theories in fashion to-day will in short time succumb in their turn, and he concludes that they are absolutely in vain (1905, 160).

Despite disagreement over its structure and precise aim, PMI is broadly understood as a historical challenge to scientific realism. It is even considered by many to be *the* strongest challenge against scientific realism (see Worrall 1989).

PMI became a key argument in the scientific realism debate with Larry Laudan's now classic *Confutation of Convergent Realism* (1981). There, Laudan provides a list of past scientific theories which were predictively successful but are now discarded. His list includes the 'humoral theory of medicine', the 'phlogiston theory of chemistry' and the 'caloric theory of heat' (33). There is more than one version of PMI. I briefly touch on two.

One version treats PMI as an *inductive* argument from the rejection of many past predictively successful scientific theories to the prospect of current – sometimes future, scientific theories (Wray 2018, 69). If these past successful theories have turned out to be false, the argument goes, then it is very likely that current – and even future, successful scientific theories are also going to be, false. This version of PMI is treated as an argument independent of NMA. Qua an *inductive* argument, its cogency rests on the strength of its inductive base (Psillos 1999, 105). In line with this remark, Peter Vickers after careful consideration of six of what are taken to be some of the most serious historical challenges against scientific realism (e.g. Fresnel theory of light, Kirchhoff's theory of the diffraction) notes that "if the historical challenge is supposed to be a pessimistic induction, the realist is in a strong position since the inductive base looks rather weak" (2018, 120).

Timothy Lyons (2002) notes that construing PMI inductively gives the impression, already expressed by Vickers, that this argument is not sufficiently supported. A better version of the argument, one, he claims, that is more faithful to Laudan's original intent, is to construe PMI non-inductively. On this version, PMI does not aim to tell us about the status of current or future theories. Instead, it is simply aims to sever the link between empirical success and truth which NMA purport to establish. Lyons calls his version the 'pessimistic meta-modus tollens'. Here is one formulation.

Premise 1: if the realist's claim is true (that success is a reliable guide to truth) then each successful theory is true.

Premise 2: There is a list of theories which were historically successful but are no longer considered true.

Conclusion: the realists' claim is false.

Consequently, NMA is undermined. Vickers's response is that this version of PMI simply misses the mark because realists use NMA as a defeasible inference, which, he holds, can survive one or two historical counter-instances (2018, 120). Elsewhere, he considers raising the bar for what counts as the kind of success warranting a NMA. There he notes that we should not be content with novel predictions simpliciter, but with *risky* novel predictions (2019). But such a response was anticipated by Lyons who notes that it will not do to claim that NMA is defeasible. For this version of PMI, unlike its inductive counterpart, is a deductively valid argument to the conclusion that NMA is unacceptable (2018, 146).

But details of these disagreements aside and granting the cogency of NMA, it seems that the argument for Putnam simply misses the mark. That is because NMA as introduced by Putnam was intended as an argument to support his tripartite scientific realism. But it should be clear that NMA, either in its earlier formulation or in its later formulation, does not seem to touch the convergence thesis. For one can entertain that success, under certain conditions, entails truth, which arguably presupposes reference, and as such accept that NMA gives support to 1. and 2. However, NMA leaves Putnam's third thesis unsupported, thereby leaving Putnam's own brand of scientific realism vulnerable to objections targeting the convergence thesis. One such objection comes from Thomas Kuhn and Paul Feyerabend's incommensurability thesis.

3.2.2. Convergence and Semantic the Incommensurability Thesis

Incommensurability, which literally means "the absence of common measure", was made popular in philosophy of science by Paul Feyerabend (1962) and Thomas Kuhn (1962), and was used differently by each, although with some overlap.

On the one hand, Feyerabend introduced incommensurability of scientific theories as a response to the logical empiricists' account of reduction and explanation. His argument was that there are no logical relations that would allow for the direct comparison of the content of different theories because there are semantic differences in terms belonging to these theories (67-69). An example that Feyerabend, and Kuhn, gives is the concept of "mass" which figures both in classical and relativistic physics. According to classical physics, Feyerabend explains, mass is 'absolute' in the sense that the concept is attributed to a system independent of its motion in the coordinate system chosen, while according to relativistic physics, it is a *relational* concept that depends on the coordinate system (80).

On the other, Kuhn introduced incommensurability in the context of challenging the cumulative nature of scientific progress. For him, incommensurability is a multifaceted relation between *paradigms* – his chosen unit of philosophical analysis. Incommensurability for Kuhn includes a semantic, methodological and perceptual dimension (1996, 148). What concerns us here is the semantic dimension. Also, despite important differences between Feyerabend and Kuhn's versions of semantic incommensurability – the former is often read as advocating radical incommensurability whereas the latter is often read as eventually accepting a partial one, treating them as advocating the same thesis does not betray our aim.

Following this brief rendering, it starts to become clear why semantic incommensurability constitutes a threat to the convergence thesis. That is because semantic variance between subsequent theories directly challenges the referential continuity thesis. And if referential continuity cannot be secured then neither can theory convergence. As such, Putnam's scientific realism is undermined.

3.2.3. Causal Theory of Reference Response

To answer that challenge, Putnam developed what he called a "causal account of physical magnitude terms" – what later came to be known as his Causal Theory of Reference (CTR), which aimed to establish two things (1979, 197):

1. Concepts which are not strictly true of anything may still refer to something.
2. Concepts belonging to different theories can refer to the same thing.

Putnam's CTR differs from its descriptive counterpart in, at least, one important respect. For Putnam explains that whereas for the latter account, intension determines extension and as such reference is theory-dependent leading to the claim that a change in theory is likely to lead to a change meaning and reference, the former account circumvents this problem by providing a theory-independent account of meaning according to which reference is secured by a causal chain of events, hence it is a *causal* theory (199).

CTR, according to Putnam, begins with what he calls an "introducing event", which involves an "*approximately correct definite description*" allowing one to acquire the ability to use a particular concept (200). Reference in other contexts, accordingly, is secured in virtue of the fact that all subsequent use of this concept will be *causally* connected to the introducing event. This is true even if the description turns out to be radically faulty, as long as it is a description of something and 'not nothing at all'. An example here will make things clearer.

The concept of electricity, Putnam explains, could be said to have been *introduced* by Benjamin Fraklin during his famous experiment. If asked about what that concept is supposed to refer to, Franklin would likely have answered that it refers to something that satisfies a list descriptions, such as that it behaves like liquid in certain respects, collects in clouds, after reaching a critical point flows from cloud to earth in the form of a thunderbolt,

etc. Following the introducing event, I may teach the word “electricity” to someone by providing a description that is radically different than Franklin’s. I may, for instance, speak of positive and negative charges and electrons which satellite the nucleus of an atom. Here, despite difference in intension, Putnam argues, I would be still referring to the same ‘thing’ that franklin spoke of, because my use of the word is causally connected to the introducing event (200).

Prima facie, Putnam’s CTR seems like a good antidote for the incommensurability thesis. However, upon a closer inspection it turns out that it also brings with it an important difficulty. Namely, Putnam’s theory seems to be an intuitively plausible account when it comes to observables, such as tigers, gold, and even electricity, but it is difficult to see how it can be generalized to unobservables, which is, arguably, the crux of the contemporary scientific realism debate.

3.2.3.1. Benefit of the Doubt Principle

In order to solve this issue, Putnam supplemented his CTR with the “Principle of the Benefit of Doubt” (PBD) – what he also called the “principle of charity” (274). But in order to explain that we must first introduce another crucial element into Putnam’s semantic system, that is his idea of “linguistic division of labour”.

One thesis that Putnam’s CTR is meant to challenge was that meanings are psychological states, which, according to Putnam, has long confused people into thinking that it is through intension that extension is secured. In Fregean terms, Putnam’s theory, we explained, aimed to explain how reference is independent of sense. Once meaning is liberated from being a psychological state, it becomes no longer a property of an individual but of a linguistic community. As such, it becomes possible, perhaps even necessary, to have a linguistic division of labour. Accordingly, we give up our right to assign words to our denotations, entrust experts with the task of doing that for us. Putnam gives the example of being able to distinguish between elm and beech, he explains that although he, on his own, is unable to tell these apart, it is through cooperation with the expert, i.e. through linguistic division of labour, that he can (275).

Having entrusted our experts with the task of assigning words to our denotations, Putnam explains, we should also give them the benefit of the doubt. Accordingly PBD is the claim that

> we should give the dubber, or the relevant expert, if the person at the other end of the chain of transmissions or cooperations isn't the original dubber, the benefit of the doubt in such cases by assuming he would accept reasonable modifications of his description (275).

Putnam provides two reasons for maintaining PBD. The first one turns on ‘the meta-induction from error’ which is none other than Larry Laudan’s Pessimistic Meta-Induction

(PMI) (1981). The gist of this argument, particularly when it is concerned with reference, is that if terms referring to unobservables according to the science of one generation turn out to be non-referring according to the science of the next generation such in the case phlogiston, then it is compelling to argue that terms we consider as referring to unobservables in our current science will turn out to be non-referring according to future science (1978, 24).

Putnam thinks that we should accept PBP on the very grounds that it blocks PMI, he explains that "It must obviously be a desideratum for the theory of reference that this meta-induction be blocked; that is one justification for the principle of benefit of the doubt" (25). However, such pragmatic justification clearly begs the question against the scientific antirealist who uses arguments such as PMI and the likes to challenge the realists.

Putnam's second strategy takes on a sceptical line of thought. It questions the pertinence of the observable/unobservable distinction when it comes to reference, arguing that if we reject PBD, then the former, i.e. observables, fair no better than the latter, i.e. unobservables. He writes that

> Electrons exist in every sense in which chairs (or sensations) exist; electron talk is no more derived talk about sensations or "observable things" than talk about sensations or chairs is derived talk about electrons (1982, 198).

Notice that this objection goes back to a time where Putnam did not yet accept direct realism. Here he was still committed to the claim that talk of external world in both its observable and unobservable compartments is nothing but derived talk about sensation. However, once one accepts direct realism, such an objection dissolves. For according to direct realism, we have said, there is no interface, and as such talk about observables is not derived talk of sensation but *direct* talk of external objects and their features which is not the case for unobservable which rely on representations.

Thus it seems that PBD is no longer a good strategy for Putnam given his conversion. Also, given that CTR on its own is not enough to maintain the convergence thesis. What does that mean for scientific realism? What that means is simply that we have no good reasons to accept any form of *convergent* realism, but that is a far cry from rejecting all forms of scientific realism. NMA, or some updated form of it, can still give reasons to accept the reference and truth thesis, and these are arguably enough to maintain a minimal form of scientific realism that accepts that science does teach truths about the mind-independent world, even if that means forgoing the linear account of scientific progress for something that is more fitting. We shall now discuss Putnam's second sense realism before providing an assessment of Putnam's broad scientific realist project.

3.3. Scientific Realism as Metaphysics

The second sense of scientific realism is one which is intertwined with metaphysical realism. Indeed it could even be said to be identical to it. Putnam certainly says at one point in his career, and as this discussion unfolds it will be clear his later views reveal that scientific realism cannot but be metaphysical. (197). Yet, what that means will remain ambiguous until we explain what Putnam means by Metaphysical realism – as will be clear Putnam's own use of the term has undergone an evolution.

In *Reason, Truth and History* (1981), Putnam characterized metaphysical realism as a cluster of three theses, which are:

Ontology: the world consists of some fixed totality of mind-independent objects.
Ideology: there is exactly one true and complete description of 'the way the world is'.
Correspondence: truth involves some sort of correspondence relation between words or thought-signs and external things and sets of things (49).

Scientific realism as metaphysical realism has been severely attacked by Putnam using what came to be known as the Model-Theoretic Argument (MTA). MTA begins by giving metaphysical realism a model-theoretic treatment. It assumes that we managed to formalize our language up to some level of Tarskian hierarchy. We would have the individual constants symbols, predicate symbols, and function symbols of that language, along with a function that assigns interpretation to all of these. Such function allows us, for instance, to assign

> Aristotle to the individual constant "a," the class of cats to the predicate "C(*x*)," the class of ordered pairs (<*x*,*y*>| *x* is a parent of *y*} to the predicate "P(*x*,*y*), " and so on (1995, 352).

Using the same function, Putnam explains, also following Tarksi, we can arrive at more complex predicates such as "(∃*x*)(C(*x*).P(*x*,*y*))" which has as an extension the class of only those *y*s such that *y* is the offspring of a cat. Having had that set, one can then define the set of "true" sentences of that language (352).

Notice that model-theory provides a crisp way of characterizing metaphysical realism. The two even seem to fit like hand in glove. That is because to accept that there is a fixed number of objects on which we map the words of a formal language is to already accept the first thesis of metaphysical realism, i.e. that the world consists of a totality of mind independent objects. Also, to accept a fixed set of properties belonging to these objects is to accept the second metaphysical thesis, i.e. that there is a single true and complete description of that world. Now given that we have a fixed set of objects, and a fixed set of properties belonging to these objects, both of which our formal language is supposed to perfectly map onto, the third thesis of metaphysical realism follows naturally, i.e. that truth involves a unique correspondence relation between our chosen language and external things.

Note, however, that metaphysical realists need not to be committed to the existence of models. The latter can be used as no more than a convenient tool to talk more precisely about the relation between word and world. In fact, the convenience of model-theory in metaphysics has led some to claim that the model-theoretic approach is the *standard approach* of doing "fundamental metaphysics" (Dorr 2010). Following this brief stage-setting we can move to the model-theoretic argument itself.

Having given metaphysical realism a model-theoretic treatment, what we end up with is a 'theory' of the world. Yet, proponents of metaphysical realism, for the most part, operate within what Putnam has called Cartesian cum materialism, or simply indirect realism, which we have already discussed at length in the previous chapter so we need not to rehearse what we said earlier. But it is helpful to remind ourselves that according to this conception we never have direct access to external objects.

Such lack of access raises for the metaphysical realist the following worry: how can we be sure that our language is *about the world*, and is not some artefact of our conceptual schemes? Put plainly, how are we able to secure reference? Putnam fuels this worry with his Model Theoretic Argument (MTA), of which what follows is a rendering (1981).

3.3.1. The Model Theoretic Argument

Consider the following sentence:

> (1) A cat is on a mat. (Here and in the sequel 'is on' is *tenseless,* i.e. it means 'is, was, or will be on'.) (33).

This sentence is true under normal interpretation, in those possible worlds where there is at least one cat, at least one mat and the cat is on the mat at at least one point in time. Also, 'cat' here refers to cats, and 'mat' refers to mats. Yet Putnam argues that this very sentence can be reinterpreted so that in the *actual world*, 'cat' refers to cherries and 'mat' to trees, without affecting the truth value of the sentence in any possible world (33).

With the new interpretation, (1) will come to mean:

> (1*) A cat* is on a mat*.

Now consider three cases where:

a. Some cat is on some mat, and some cherry is on some tree.
b. Some cat is on some mat, and no cherry is on any tree.
c. Neither of the above.

We now provide a definition of 'cat*' and 'mat*':

Definition of 'cat*': x is a cat* if and only if case a. holds and x is a cherry; or case b. holds and x is a cat; or case c. holds and x is a cherry.

Definition of 'mat*': x is a mat* if and only if case a. holds and x is a tree; or case b. holds and x is a mat; or case c. holds and x is a quark.

In possible worlds falling under a. where 'a cat is on a mat' is true, 'a cat* is on a mat*' is also true because in such worlds cherries are cats* and trees are mats* and in this world some cherry is on some tree. Now given that the actual word also has some cherries on some trees, then the actual world is similar to a. in this respect and, as such, in the actual world cat* refers to cherry and mat* refers to trees. In possible world falling under b. where 'a cat is on a mat' is true, 'a cat* is on a mat*' is also true because given the definition 'cat' and 'cat*' are coextensive in b. and so are 'mat' and 'mat*'. Also, although cats are cats* in some worlds, Putnam explains, they are not cats* in the actual worlds because unlike the actual world, b. assumes that there is no cherry on any tree. As for possible worlds falling under c. where 'a cat is on a mat' is false, 'a cat* is on a mat*' is also false because a cherry cannot be on a quark (34).

What follows from the foregoing is that in *all possible worlds* a cat is on a mat if and only if a cat* is on a mat*. Hence, by reinterpreting 'cat' so as to give it the intention assigned to it by 'cat*', and by doing the same for 'mat' and 'mat*', we are able to make 'a cat is on a mat' mean 'a cat* is on a mat*' without affecting the truth value of 'a cat is on a mat' in all possible worlds (35).

Putnam notes that a complicated variant of such an exercise can be carried out for the sentences of whole languages leading to conclusion that

> there are always infinitely many different interpretations of the predicates of a language which assign the 'correct' truth-values to the sentences in all possible worlds, *no matter how these 'correct' truth-values are singled out* (35).

In brief, if MTA is true then settling the truth-value of every sentence in a language is insufficient to determine the reference and correspondence relations that proponents of metaphysical realism – who also happen to be indirect realist in perception, need for their position to be secured. Given that MTA plays a central role in the later Putnam's natural realism. Indeed, as I have explained in section 1.2. it is the MTA that got the later Putnam to accept direct realism in perception – and forced to him to reconsider the status of common sense objects, as a way to ward off its sceptical conclusions. But before that and before we even consider Putnam's internal realist solution, it is helpful to consider some of the responses that tried to take on MTA heads-on.

Given that the sceptical conclusion of MTA is that reference is radically indeterminate, a natural response was to try to place constrains on reference. The idea here is that if we can

manage to place more constrains on reference then we will be able to significantly reduce the number of acceptable interpretations. This idea comes in all shapes and sizes, two examples of which are David Lewis's (1986) distinction between genuine and pseudo-properties and relations and the more famous causal constraint strategy (Glymour 1982, Devitt 1983, Lewis 1984). I explain each in turn. Then I turn to Putnam's response to them before assessing the overall cogency of MTA.

A helpful way to mitigate MTA, Lewis suggests, is to make a distinction between genuine and pseudo-properties and relations. The idea behind this is that model theory provide us with a far too many pseudo-properties and relations whereas genuine properties and relation are far less abundant than proponents of model theory would have us believe. As Lewis explains:

> Sharing of [genuine properties] makes for qualitative similarity, they carve at the joints, they are intrinsic, they are highly specific, the sets of their instances are *ipso facto* not entirely miscellaneous, there are only just enough of them to characterise things completely and without redundancy (1986, 60).

If we insist that the interpretation of a predicates must pick up genuine properties then we need not worry about MTA for only genuine properties are 'referentially magnetic'. The problem with this approach is that this seems to be asserted without argument. If we are to use the joint metaphor, it is not clear why reality cannot have more than one set of joints at which it can be carved. And if it possible that reality can have more than one, it is not clear why it cannot have many. Consequently genuine properties on their own would not be enough to fix reference even if they help to reduce the number of acceptable interpretations. But even that is not too helpful as we do not know how many joints reality has. As such merely appealing to genuine properties ultimately falls short of helping us to mitigate MTA. The second approach is to appeal to causal constraints.

Similarly to Putnam's own response against incommensurability as discussed in section 3.2.3., some metaphysical realists attempted to add causal constrains on reference by adopting CTR. So the intended interpretation would have to respect *all* the causal connections between the word and object. Putnam, however, anticipating such a response, argued that CTR fails to mitigate MTA because CTR is 'just more theory'. He explains that

> The problem is that adding to our hypothetical formalised language a body of theory entitled "Causal theory of reference" is just adding more theory […] If 'refers' can be defined in terms of some causal predicate or predicates in the metalanguage of our theory, then, since each model of the object-language extends in an obvious way to a corresponding model of the metalanguage, it will turn out that, *in each model M, reference* $_M$ is definable in terms of *causes*$_M$; but, unless the word 'causes' (or whatever the causal predicate or predicates

> may be) is already glued to one definite relation with metaphysical glue, this does not fix a determinate reference for 'refers' at all (1980, 477).

In simple terms, how “causes” in CTR can have a unique interpretation is no less puzzling than how ‘cat’ can have a unique interpretation in the example above. Hence, Putnam holds, invoking CTR is susceptible to the same challenge raised against any other theory or model of the world, and as such it is ‘just more theory’ – this response of Putnam, it should be added, is general and applies equally to all forms of constraints including Lewis’s appeal to genuineness.

But metaphysical realists have pushed back against Putnam’s just-more-theory manoeuvre. When it comes to causal constraint, they held that Putnam failed to distinguish an interpretation’s *modeling* a constraint, i.e. making a *statement* expressing the constraint – which depends on language and as such is susceptible to MTA, and an interpretation’s actually *conforming* to that constraint which does not depend on language. They held that the just-more-theory manoeuvre attacks the former whereas what is intended by the causal constraint is the latter (Lewis 1984, 224-225). I elaborate.

If we accept a causal constraint C and a sentence S expressing C in a language L given an interpretation I and referential relation R and we ask the following question: would it not suffice that S comes out as true given I for us to maintain that I conforms to C? Proponents of the causal constraint strategy think not. As Hale and Wright explain, one thing that may vary under different interpretations of L is the referential relation R, so we cannot assume that S will simply express C. Under some interpretations S will express C but under many more it will not. Also, under an interpretation J, S may still come out as true even when it fails to express C. They add that if we can express C under some other language G that has the semantic resources allowing it to discuss the semantics of L, then we would be able to say that even though S is a sentence of L true under J, J does not conform to C (2017, 715). Hence, since making a statement expressing the constraint is different than actually conforming to the constraint, and since Putnam’s target in his just-more-theory manoeuvre is a theory that includes the statement of the constraint, it seems that Putnam’s just-more-theory manoeuvre misfires. But is that so?

If we look at the example above, we notice that its success in fending off Putnam’s just-more-theory manoeuvre is conditional on being able to have a language G fit for purpose. Particularly, the reference of expressions in G needs to be determined enough to allow us to give an expression of the constraint C. It is at this point, however, that Putnam is able to run the just-more-theory response. That is because G just like L, just like any other language for that matter, will have to confront the same problem of the determinacy of reference and as such we are back to square one.

It seems to me that when some proponents of the causal constraint complain that Putnam’s just-more-theory manoeuvre misfires because it confuses the term “causation” and

causation itself they fail to appreciate the depth of Putnam's response. Recall that Putnam central question is *not* how our words can have reference given that we have an already fully functional language in place. Instead, his question is how our words, any and all words, can have reference at all ab intio given a particular metaphysical picture. Hence proponents of causal constraint response cannot have recourse to a metalanguage which itself is equally susceptible to MTA.

Many have noticed that Putnam is not simply begging the question against the metaphysical realist, but instead that he is presenting metaphysical realists with a sceptical challenge that threatens to undercut their whole metaphysical picture. Hale and Wright rightly note that as soon as metaphysical realists take up the challenge to explain what constitutes a determinate relation of reference, Putnam can interrupt and complain that they have no right to assume that their words have a determined reference. Yet instead of acclaiming what seems to me the closest one can get to knock-down argument, they grumble that Putnam's just-more-theory is 'boring' and 'jejune'. They protest that

> Obviously the metaphysical realist has to be presumed capable of contentful – so, determinately referential – speech if he is to respond to Putnam's challenge, or indeed to any challenge at all (2017, 716).

But to me at least, and I take it to many others sympathetic to Putnam's MTA, it is not obvious at all why the metaphysical realist should be presumed capable of contentful speech, especially when this is exactly the point that MTA is meant to challenge. Recall that MTA itself was intended as a reductio against the metaphysical realist picture and if semantic scepticism follows from metaphysical realism then we should accept that MTA is vindicated. In what follows I consider the middle-Putnam's response to MTA.

3.3.2. The Internal Realist Solution

According to the middle-Putnam, there are two ways to avoid the threat of MTA (1980, 464):

1- Extreme Platonism
2- Internal Realism

The first position, according to Putnam, adopts a "magical theory of reference" (1981, 3) by either reverting to 'medieval essentialism' (1985, xii), relying on 'divine intervention' (1984, 7), or positing occult 'neotic rays' which connect words and thought-signs to their reference (1981, 51). All of these, however, are dismissed by Putnam as they prove too supernatural for his naturalistic liking.

The other option, which the middle-Putnam offers, and adopts, is internal realism, the meaning of which, Putnam concedes, suffered self-inflicted ambiguity, not to mention that the position itself has undergone some flux. As initially introduced by Putnam, internal

realism was supposed to capture a kind of realism that is internal *to science* – hence *internal* realism, which both metaphysical realists and antirealists can agree on despite their metaphysical disagreements (2012, 56).

The point behind it is that if scientific realism is an *empirical theory*, as claimed by Putnam, then it could only be shown to be incompatible with (metaphysical) antirealism such as that of the positivists or van Fraassen if it could be shown that those actually reject at least some scientific theories. Otherwise, scientific realists would be simply begging the question on the antirealists (56). And since van Fraassen and other scientific antirealist do not strictly reject scientific theories, then even those turn out to be internal realist on Putnam's minimalist account.

No sooner, however, did internal realism itself turn, at the hands of Putnam, into a metaphysical position which rejects metaphysical realism as construed above – with its three tenets of ontology, ideology and correspondence. Internal realism, under the weight of MTA, also subscribed to the claim that truth cannot be radically non-epistemic. That is truth cannot be independent of all means by which we verify it.

What Putnam drew from MTA was that if neither our theoretical constraints nor our observational constraints are able to secure the right interpretation then reference is radically indeterminate, and if that is the case then truth as correspondence, as construed above, cannot be salvaged. Given that Putnam is keen on maintaining the notion of truth, he was left with two options, either to dilute the notion by perhaps accepting a pragmatic theory of truth – an option he rejected because he thought that truth is too important to be diluted as such (2016, 91) or to accept an epistemic notion of truth. He opted for the latter.

3.3.2.1. Epistemic Notion of Truth

Accordingly, the epistemic notion of truth that middle-Putnam subscribed to what truth as "idealized rational acceptability" or "verification under ideal conditions" – both terms for Putnam amount to the same thing (2012, 53). 'Idealized' or 'ideal', here, play an essential role, for Putnam explains that truth cannot simply be 'rational acceptability', that is because "that earth is flat" may have been rationally acceptable 3000 years ago, but it is not rationally acceptable today. Yet, we would not say that that earth is flat was *true* 3000 years ago but is no longer true today. Putnam's example is meant to capture an essential feature of truth, namely that it, unlike justification, is a property of statements *that cannot be lost*. Also, unlike rational acceptability, it is neither tensed, nor relative to persons (55). Having clarified that, an important question follows. Namely what do these ideal epistemic conditions amount to? Here, Putnam's views vary.

Initially, Putnam's view was that 'ideal epistemic conditions' are like "frictionless planes", in that though we are unable to attain them or even be sure that we have come sufficiently

close to them, we can, nonetheless, be sure that we are approximating them. The take-home of this simile is twofold:

1. Truth is independent of justification here and now, but not independent of *all* justification. To claim a statement is true is to claim it could be justified.
2. Truth is expected to be stable or 'convergent'; if both a statement and its negation could be 'justified', even if conditions were as ideal as one could hope to make them, there is no sense in thinking of the statement as *having* a truth-value (56).

3.3.2.1.1. Rethinking Ideal Conditions

Later, however, Putnam changed his mind about how we should understand ideal conditions. Although, the initial characterization betrays some similarity to Peirce's 'end of enquiry', Putnam later became keen to distance himself from such a 'utopian' characterization. His new construal of these ideal conditions is closer to common sense where he explains that

> If I say 'There is a chair in my study", an ideal epistemic situation would be to be in my study with the lights on or with daylight streaming through the window, with nothing wrong with my eye-sight, with an unconfused mind, without having taken drugs or been subjected to hypnosis, and so forth, and to look and see if there is a chair there (1992, viii).

In the same paragraph, Putnam concedes that 'ideal conditions' serves as no more than a metaphor which we can drop and opt for more literal expressions such as being in "better and worse epistemic situations with respect to particular statements" (viii).

Putnam's new common sense understanding of ideal epistemic conditions, however, rendered internal realism susceptible to a host of glaring counterexamples. Putnam's favourite example is the claim that *N:* "there are no intelligent extra-terrestrials" (2016, 132). Such a statement, if it is true, Putnam explains, cannot be verified. Or take the more elaborate example, the statement *T* which is the conjunction of *N&S&P*. We already know what *N* is. *S* is the claim that an observer cannot verify whether there is life in space-time regions where he is unable to receive causal signal from. *P*, Putnam explains, is a "possible empirical theory", which includes a number of statements:

1. Causal signals do not travel faster than the speed of light.
2. It is physically possible that there is extra-terrestrial life.
3. It is also physically possible that there is not.
4. There are large space-time regions where possible observers are unable to receive causal signal from.

Putnam explains that if *T* were indeed true, it would be *logically* impossible to verify (134). Counterexamples such as this one rendered internal realism extremely suspect. Also, for

Putnam, internal realism's verificationist account of truth is coupled with a verificationist account of *understanding*.

> the brain's 'understanding' of its own 'medium of computation and representation' consists in its possession of a *verificationist semantics* for the medium, i.e. of a computable predicate which can represent acceptability, or warranted assertibility, or credibility (1985, 142).

Later, however, Putnam came to hold that verificationist accounts of understanding inevitably leads to solipsism – a conclusion which clashes with internal realism's being a form of *realism*. That is because if the only substantive notion of understanding available to me is that of *being verified*, then talk of other people is only intelligible to *me* since it uses statements that will only be verified by *me*, hence solipsism (2012, 79).

3.4. Metaphysical Realism after Internal Realism

Given such difficulties, and others, Putnam found himself forced to give up internal realism, understood in its narrow sense as a position on truth, and to seek an answer to MTA beyond 'supernatural theories of reference' and internal realism. We noted in the previous chapter that Putnam found that in adopting direct realism in perception. The solution in a nutshell can be stated as the claim that we are able to secure reference to 'external object' because we directly perceive them. There is no interface between word and world that may cast doubt on our referential abilities.

Notice, however, that Putnam's last move secures access to the world but does not rescue Metaphysical realism as construed above. That is because when accepting that sense perception gives us direct cognitive access to the world we are accepting that perceived object and properties are *real* parts of the world (think of intentional properties such as reference and justification, etc., which Putnam has argued at length are irreducible to physics but also of what modern philosophers called secondary properties such as colours, sounds etc.) Yet such objects and properties have no place given Metaphysical realism which takes as real only those objects and properties that belong to fundamental physics. This leads Putnam to reject Metaphysical realism. His argument can be summarized as follows:

1. If sense perception gives us direct access to the world then objects and properties that we get at using sense perception are real unless they can be shown to be reducible to physics.
2. All attempts to reduce such properties have so far failed with no sign of success in the offing.

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3. Perceptual properties are real (including the object bearing them)

And

4. If Metaphysical realism is true then perceptual properties are not real
5. But perceptual properties are real

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6. Metaphysical realism is not true

Putnam conceded that, having given up internal realism, he turned into *a* metaphysical realist. Yet he was quick to note that "metaphysical realism" can be used to denote different positions. The one that he subscribes to is a metaphysical realism understood in the broad sense of rejecting epistemic conceptions of truths and claims such as the world is of our own making (2012, 101). It turns out that the kind of metaphysical realism that Putnam has long opposed amounts precisely to the denial of a phenomenon he calls "conceptual relativity" (62). Given that the latter concept is central to Putnam's later views on realism, we shed more light on it in what follows.

3.4.1. Conceptual Relativity

What we have been so far discussing is the second sense of scientific realism which Putnam equates with Metaphysical realism. The latter (with a capital M), we said is captured by the three theses of ontology, ideology and correspondence. In what follows, we will explain what, for Putnam, conceptual relativity amounts to. But for now, the take home message from the foregoing discussion is that the second sense of scientific realism, which is scientific realism as metaphysics, amounts to the denial of conceptual relativity which, as we will see, Putnam rejects.

Putnam characterizes conceptual relativity as the thesis that "there are ways of describing what are (in some way) the 'same facts' which are (in some way) 'equivalent' but also in some way 'incompatible' (1987, 29). Conceptual relativity is meant to capture the "interpenetration of facts and conventions" (1995, 58), where a convention "is simply a *solution to a certain kind of coordination problem*" (2004, 44). So while there is, in everything that we take to be true, an element of factuality and an element of conventionality, Putnam holds, we would be committing a fallacy if we conclude that there are therein elements that are entirely conventional and others that are entirely factual (1992, x).

To explicate his notion of equivalence Putnam uses the Wittgensteinian's notion of 'grammar'. He writes that the realist who values the belief in one true theory when confronted with different, even incompatible, theories, can maintain that such prima facie incompatibility exist only at the level of 'surface grammar', and that at a 'deeper level' these theories are in fact one (1985, 27). Later, Putnam further clarifies what he means. He notes that two description (theories) are cognitively equivalent when the phenomena which one theory explains in one 'optional language' can be explained in another 'optional language' where optional languages resemble something like conceptual schemes which are not indispensable for the mastery of a natural language – hence they are 'optional' (2001, 434).

Accordingly, Putnam maintains that

> what logicians call "the existential quantifier," the symbol "(∃*x*)," and its ordinary language counterparts, the expressions "there are," "there exist" and "there exists a," "some," etc., *do not have a single absolutely precise use but a whole family of uses* (2004, 37).

Of course these different uses will have to obey the same logical laws, but such laws are not enough to fully determine the use of these expressions. Also, such remarks are not restricted to the existential quantifier, for Putnam holds that they equally apply to the universal quantifier "(*x*)" (38), to what counts as an "individual" and "object", etc. the meaning of these words and expressions is something that the natural language leaves entirely open (43). Conceptual relativity understood as the 'Indeterminacy of Optional Language', Putnam explains, is pervasive in science, and can be illustrated with a few examples.

3.4.1.1. The Case of Mereological Sum

Putnam's favourite example comes from mereology which is "the study of the relationship of part to whole" particularly within the context of formal ontology. It involves the notion of 'mereological sum' which is characterized as a whole consisting of its composite parts regardless of the distance between them (Blackburn 2005). So for illustration Putnam gives as an example of a mereological sum the object consisting of his nose and the Eiffel tower (2004, 36).

Putnam then invites us to consider a world which contains three individuals x_1, x_2, and x_3 which are not decomposable within the designated universe of discourse – Putnam calls this the 'world of Carnap'. This universe contains 3 *objects*. Now Putnam notes that applying mereology to this universe and ignoring what is called the 'null object' we get the *same* universe would contain 7 instead of 3 objects:

World (*a la* Carnap)	World (*a la* Lezniewski)
x_1, x_2, x_3	x_1, x_2, x_3
	$x_1 + x_2$, $x_1 + x_3$, $x_2 + x_3$
	$x_1 + x_2 + x_3$

Here we have two prima facie incompatible claims about the same world. One says that the world contains three objects, while the other says the world contains seven. Putnam notes that this incompatibility cannot be resolved by merely saying that world *a la* Lezniewski includes mereological sum in its universe of discourse whereas world *a la* Carnap does not. That is because the difference here has less to do with the universe of discourse and more to do with the very meaning i.e. use of the word 'exist'.

Putnam's point is quite metaphysical. That is why perhaps, given his aversion to metaphysic, he has such a hard time expressing it. But we can say that Putnam's question is *ontological*. It concerns what the *real furniture of the world* is. Are there *really* three or seven objects in this world? Putnam's answer to that question is "it depends". It depends on how *we* use the word 'exist' and 'object'. If we choose the 'Carnapian optional language' then there are three objects in the world, if we choose the 'Lezneiwskian optional language' then there are seven objects in the world. But fundamentally it is the same state of affairs described using to different optional languages. Deciding which is true, Putnam holds, is literally a matter of convention (2004, 43). This example is impressive only in so far we are willing to take talk of mereological sums to have ontological significance, which not many are willing to do. But Putnam tells us that the phenomenon of conceptual relativity is also pervasive in the empirical sciences. Here are two examples:

3.4.1.2. The Case of Physical Dualities

In *Mathematics without Foundations* (1967), Putnam presents the wave-particle duality in quantum mechanics as a case of conceptual relativity – although he had not yet coined the term. Back then, he called it following Reichenbach 'equivalent descriptions'. Putnam explains that

> The description of the world as a system of particles, not in the classical sense but in the peculiar quantum-mechanical sense, may be associated with a different picture than the description of the world as a system of waves, again not in the classical sense but in the quantum-mechanical sense; but the two theories are thoroughly intertranslatable, and should be viewed as having the same physical content (8).

So when we say that an electron is a wave with a definite wavelength λ, or we say that it is a particle with a momentum *p* and an indeterminate position, Putnam holds that at bottom we are expressing the 'same fact' or the '*same state of affairs*'. Putnam admits that what 'the same fact' amounts to is unclear, but insists that the relation between the two sentences is not of synonymy, in that these different expressions do not linguistically mean the same thing although they express the same underlying states of affairs. Using Putnam's more recent lexicon we can say that talk of electron as wave or particle are optional languages. They express the 'surface grammar' used to describe the same underlying states of affairs (8).

A more recent example (2012) also comes from quantum mechanics. There Putnam notes that the phenomenon of conceptual relativity is known amongst physicists under the label 'duality', citing in support a (1994) paper by Burgess and Quevedo entitled "Bosonization as Duality". Putnam explains that what analytic philosophers call the 'ontology' of a conceptual scheme, particularly in the context of a quantum mechanical theory of a particular target system, is not the 'load-bearing aspect' of the scheme. That is because the scheme – I believe Putnam should have said 'state of affairs' or 'facts' instead of scheme here, can have

many different *representations* all of which are perfectly equivalent (2012, 57). Whether or not we want to include bosons in the scheme is a matter of convention.

Hence, conceptual relativity for Putnam captures the phenomenon whereby the same underlying state of affairs can be captured by different representations, which although perfectly intertranslatable, *do not preserve ontology*. Instead, what they preserve is the *explanation of* the *same phenomenon*, where what counts as phenomenon is to be left to the physicists (57). Importantly, conceptual relativity, according to Putnam, is a special case of a wider phenomenon which he calls 'conceptual pluralism' (2004, 48). Clarifying the latter is the focus of what follows.

3.4.2. Conceptual Pluralism

As a first approximation, Putnam negatively characterizes conceptual pluralism as "the denial that any one language game is adequate for all our cognitive purposes" (2012, 111). A more positive, although still rough, characterization is that a something "may be partly described in two very different vocabularies" (2004, 48). As an example, he notes that it is a true description of the world to say that some passages of Kant's *Critique of Pure Reason* are difficult to interpret and that Andrew Wiles and Richard Taylor gave a correct proof of Fermat's last theorem, although neither of these claims is translatable into the language of physics – on any reasonable sense of 'translate' (2004, 65). These are meant illustrate that there are many sorts of descriptions which we take to be true of the world even if they are untranslatable to physics.

At first, conceptual pluralism seems like an innocuous claim which only the most hardcore reductive physicalist would deny. Quine, for instance, Putnam explains, would accept conceptual pluralism, while distinguishing between a 'first-grade conceptual system' (which is science properly formalized) and our 'second grade conceptual system' – a distinction that is reminiscent of Sellars's distinction between scientific and manifest image. Only the first grade system, Quine holds, bears on the question of ontology, i.e. present an account of what the world *really* is like. That is why, Putnam continues, Quine rules out 'meaning facts', i.e. facts about meaning and reference, precisely because such facts cannot be accounted for given our first grade – scientific - conceptual system (112).

Yet, the thrust of conceptual pluralism lies in the fact that it is meant to challenge the ontological significance of this and similar distinctions, where Putnam insists that our second grade systems – to use Quine terms, have more than merely *heuristic* significance. They possess, no less than the first grade conceptual system, bona fide ontological significance. An example here will help clarify things.

Describing the content of a room, Putnam explains, may be done *partly* using the conceptual system of fields and particles, but it can also be done using the conceptual system of tables and chairs. 'Partly' here is a crucial qualification because neither

description claims to be complete, that is why these two descriptions are not incompatible, not even at face value (2004, 48). In fact these two descriptions can be understood as complementary, in that when joined together they provide a more complete, albeit still partial, description of the content of the room.

Now, it becomes clear why Putnam considers conceptual relativity to be a special case of the conceptual pluralism. That is because in addition to satisfying the basic claim of conceptual pluralism, namely that the world can be described using different conceptual systems – or language games, conceptual relativity adds another condition which is that the descriptions of the same state affairs using different conceptual systems needs to be cognitively equivalent in the technical sense of being 'mutually relatively interpretable' (2012, 65), where by 'mutual relative interpretability' of two theories T1 and T2, it is meant that given formal definitions of terms in T1 and terms in T2, there exists a possible definition of terms of T1 in the language of T2 which makes all theorems of T1 theorems of T2, and vice versa (1983, 38). But a certain ambiguity remains.

The two characterizations of conceptual pluralism above, although presumably denoting the same phenomenon, seem to be saying two very different things so as to deserve two different labels. Consider the negative characterization which says that no one language game, or conceptual scheme, etc., is adequate for all cognitive purposes. This implies that some conceptual schemes are adequate for some cognitive purposes but not others. It is very likely that while making this claim Putnam had in mind the domain of intentional discourse, i.e. discourse involving meaning, reference, etc., which Putnam, we noted, stressed cannot be reduced to physic-cum-computer science.

3.4.2.1. Levels of Form

This example, however, does not accord with the positive characterization of conceptual pluralism which says that two different conceptual schemes can be used to partly describe the same thing. The latter, positive characterization, we saw, seems to work more with describing the content of the room. What this shows is that conceptual pluralism, as used by Putnam, seems to capture more than one phenomenon, the possibility of application for which depends on the domain of discourse. From Putnam's work I can distinguish three domains.

Domain 1: where physical descriptions fail and only the common sense description succeed. An example is the domain of intentional discourse, i.e. discourse involving meaning, reference, etc.

Domain 2: where both physical and common sense description partially succeed such as in the example where we are describing the content of the room above.

Domain 3: where only the physical description succeed, or more precisely, only the physical description applies, such as in the discourse involving atoms, electrons, etc.

Interestingly, when considering the implications of saying that, for example, intentional discourse cannot be reduced to theoretical physics, Putnam notes that this does not mean that physics is false, but that "to use Aristotelian language, the world has many levels of form" (65), which each can be described using its own language game and seeks its own level of explanation (Putnam & Peruzzo 2015). This, we can say, provides grounds for Putnam's aforementioned claim that, pace Quine, secondary conceptual systems are also ontologically significant. This is due to the fact that they capture 'levels of form' which physics fails to capture.

Unfortunately, despite that this idea of 'levels of form' does a lot of work for Putnam – it underlies the whole of his conceptual pluralism, and accordingly his conceptual relativity, he does not explicitly discuss it in any detail save for a few scattered remarks. What these remarks, nonetheless, show is that, for Putnam, it is in virtue of these levels of form that his phenomena of conceptual pluralism goes beyond mere linguistic practices and is given grounds in reality. As for the reasons why Putnam simply mentions levels of form – with a shy nod to Aristotle, but without much explication, it is likely that he finds, also like Aristotle, that the notion is metaphysically and conceptually basic. In what follow I consider how Putnam's notions of conceptual pluralism and levels of form can help to think about the relation between scientific and common sense descriptions of the world.

3.4.3. The Relation between Science and Common Sense

Conventional scientific realists, I have noted, have, for the most part, assumed that scientific and common sense descriptions of the world are at least in tension and at most that scientific descriptions have shown common sense descriptions to be false (Sankey 2020). This sentiment has been pervasive amongst philosophically-minded scientists such as Arthur Eddington with his famous case of the two tables (1927) and scientifically-minded philosophers such as Wilfrid Sellars with his attempt to reconcile the 'manifest image' with the 'scientific image' (1963).

Although examples such as these speak of a tension between these two descriptions, it is obvious, for the conventional realist, that, should one of these be eliminated, it is going to be the common sense descriptions and not the scientific one. Sellars is famous in claiming that although the manifest image is certainly pragmatically useful in helping us go about our daily lives, in the course of describing and explaining the world it remains inadequate as "science is the measure of all things, of what is that it is, and of what is not that it is not" (20).

Some may wonder why philosophers of science interested in the realism debate should even bother with the question of the relation between science and common sense. After all, a glance at the literature shows that it is not much discussed, if at all, and the figures mentioned in my discussion are not really part of the *current* debate. In responding to that I can easily name a few philosophers who see scientific realism as incompatible with common

sense, such as Paul Churchland who, being an 'eliminative materialist' argues that our common sense understanding of the mind is radically mistaken and that mental states do not actually exist and have no place in a mature science of the mind (1981).

More recently James Ladyman (2007) also posited a schism between science and common sense when he claimed that

> Given that the 'common sense' of many contemporary philosophers is shaped and supplemented by ideas from classical physics, the locus of most metaphysical discussions is an image of the world that sits unhappily between the manifest image and an out of date scientific image (10).

Similarly, Steven French, while 'adopting the stance of the scientific realist', and inspired by Eddington, defends the claim that 'there are no such things as ordinary objects', all the while reassuring the common sense realist that with the right metaphysical tools we need not give up *talk* of ordinary objects and properties (2019, 260).

But I wish not to place too much emphasis on individuals and to point out instead another reason why this topic is no longer extensively debated amongst philosophers of science in general, or in the scientific realism in particular (although see a recent contribution by Peels et al. 2020). This is that the conflict narrative has become so pervasive, almost accepted as orthodoxy, that it has made it well beyond philosophical circles with one conventional realist recently claiming that if one opens any graduate-level textbook or popular science book they will immediately notice that "the picture of the world that science pushes on us is getting farther and farther away from our common-sense view of that same world" (Pigliucci 2020, 310-311). But if my discussion in chapters 1 and 2 is cogent and perception is in fact direct, that is if perception involves a relation between subject and mind-independent objects and features, then our common sense descriptions of the world which we come to by directly perceiving it cannot be easily dismissed as such.

My discussion of the later Putnam on realism reveals the relevance of perception for scientific realism. Particularly, it reveals the necessity of direct realism in perception for the *very possibility* of scientific realism. The point should be clear. The first point of encounter of scientists with the mind-independent reality prior to any form of scientific investigation is through perception. But not any account of perception will do, since for Putnam all indirect theories of perception are susceptible to MTA thereby opening the door for Kantian skepticism which puts in question the very possibility of our utterances having meaning. This undercuts any form of realism about the mind-independent reality, scientific or otherwise. What is needed, for Putnam, I said is direct realism in perception which purges Kantian skepticism as well as Cartesian skepticism. But direct realism has metaphysical implications, namely that once we accept it we should also accept that the common sense objects and their features are real, i.e. they belong to the mind-independent reality. As such, it becomes

no longer easy, as conventional realists often do, to dismiss common sense descriptions as being pragmatically useful but ultimately false since that would negate the very basis on which scientific realism is built.

What Putnam's notions of conceptual pluralism and 'levels of form' do is that they give us the metaphysical machinery to solve that issue. That is because if we accept that reality admits of levels of form in the strong Aristotelian sense, and if each form can be described using a different conceptual scheme – and with the additional phenomenon of conceptual relativity multiple conceptual schemes within the same domains, each suitable for its intended domain and neither claiming to provide an exhaustive description of reality, then it no longer is the case that common sense and scientific descriptions are incompatible. That is because although both purport to be about reality, they do not target the same 'level of form', and as such they do not present us with incompatible descriptions.

That way conceptual pluralism and levels of form give us a way to have our cake and eat it too so to speak. On the one hand, they allow us to maintain the veracity of common sense descriptions brought about by accepting direct realism in perception which made scientific realism possible. On the other hand, they also allow us to accept the veracity of scientific descriptions, carrying through with scientific realism to the metaphysical end without having to restrict ourselves to epistemic notions of truth and its ilk.

3.5. Reconsidering Scientific Realism

We have previously noted that, in the scientific realism debate, Putnam defends 'convergence realism' which is characterized in terms of his three theses Reference, Truth and Convergence. We said that Putnam uses NMA to defend it but that he has not given good reasons to accept the convergence thesis. This is pertinent, we said, especially that incommensurability challenges the convergence thesis by blocking reference continuity and accordingly theory convergence, not to mention PMI which challenges reference itself. But there are also important challenges to traditional forms of realism that Putnam raises but fails to answer and we consider those in what follows.

We said earlier that the middle-Putnam in attempting to deal with MTA, which, recall, says that, given an indirect theory of perception, if neither our theoretical constraints nor our observational constraints are able to secure the right interpretation of are words then reference is radically indeterminate, he was forced to choose between 'extreme Platonism' and 'internal realism'. This radical indeterminacy of reference was mainly due to the fact that we lack direct access to whatever we are trying to refer to. Our only access is indirect, i.e. by means of a representation.

We said that the later Putnam solved that issue by adopting a direct theory of perception. Yet this move only solves the problem at the level of 'observables' but not unobservables, for which our only access continue to be indirect, i.e. through representation even when we

accept direct realism. Hence Putnam's solution is only partial, and the problem of the indeterminacy of reference re-emerges if not as challenge for metaphysical realism minimally construed as the belief in a mind-independent world to which we have cognitive access, then as a challenge for any form of scientific realism with a reference thesis. If MTA is cogent then it seems to make false, or at least highly questionable, *almost any* reference thesis belonging to scientific realism. But the reference thesis, given Putnam's challenge, may face an even greater threat, namely that, given the phenomenon of conceptual relativity, the reference thesis may end up becoming meaningless.

Reference, as commonly understood, is a relation that obtains between representational tokens and objects. Putnam's reference thesis states that "terms in mature scientific theories typically refer", presumably they refer to objects and related features. But a central tenet of Putnam's later views is that we can and often do have 'objectivity without object' (see 2004). In light of this, it becomes not entirely clear what the reference thesis amounts to. This not only does violence to the reference thesis but also to the reference continuity part of the convergence thesis.

Also, Putnam, we previously noted, holds that the 'ontology' of a conceptual scheme, particularly in the context of a quantum mechanics, is not the 'load-bearing aspect' of the scheme. This is all well and good, but it still does not answer what the *term* refers to if not a particular object. The later Putnam might respond that it does refer to an object as it figures in our successful scheme albeit the very nature of the object and the meaning of reference here differ from cases where we are referring to ordinary common sense objects. This however, raises important questions; one which I find most pertinent is the following.

If we, given Putnam's framework, are postulating at least two ontologically distinct kinds of objects, ordinary objects and scientific objects where these too seems to be radically different then it seems unwarranted to assume, without argument, that whatever properties ordinary objects possess scientific objects also possess.

Consider ordinary common sense objects. Putnam's direct realism, we have stressed, has metaphysical implications. If we are directly aware of the external world then ordinary common sense objects and their features are real. These objects endure. That is they are generally stable under a variety of conditions. Apples, for instance, continue to exist under a variety of conditions (different lighting, temperature, pressure, etc.). We are warranted in saying that because we directly perceive them, noting their general endurance. This is not the case however for ice, which endures only under particular conditions of temperature and pressure. The emerging question becomes: are scientific objects like apples or ice? In other words, how do we know whether such objects endure? I use endure instead of persist because I wish to distance myself from the metaphysical debates about the relation between objects and time.

This question seems not to have received serious attention from scientific realists whose focus has been for the most part on the question of existence. Yet the answer to it seems far from obvious. An anticipated response is that scientific objects are like apples the justification for which runs along NMA. But the problem with such a response is twofold.

We previously noted that scientific objects seem very different from ordinary objects. In fact, some scientific realists who are of the Ontic Structural variant hold that quantum physics give us reason to think that fundamental entities cannot be individuals and as such are unlike ordinary object in at least one *fundamental* respect (French 2000). Ontic Structural Realism notwithstanding, if quantum physics gives us reason to think that fundamental particles and ordinary objects are dissimilar in one respect, it no longer seems unreasonable to think that they may be dissimilar in a host of other respects as well. And if this positive claim, modest as it is, still seems excessive. Then, for lack of good reasons, the best that we can do is to suspend judgement about other similarities, relevant to my point is endurance.

But we can do more than provide sceptical challenges. Ice seems to do very well in refrigerators and the Arctic region but not so well in the Sahara desert. By the same token NMA-type arguments for the endurance of scientific objects give us good reasons to think that they endure *when* our scientific practices succeed, i.e. in condition when success is born out. But it does not give us reasons to extrapolate beyond those and relevantly similar domains. Pertinent to this is Nancy Cartwright's recurrent remark – and this point will a come up again in chapter 4 where we discuss Cartwright scientific realist views, that

> "our most beautiful and exact applications of the laws of physics are all within the entirely artificial and precisely constrained environment of the modern laboratory" (1999, 46).

Although Cartwright's point is made for laws of physics, it is not difficult to make similar type-arguments for such objects as well, The point is that if such conditions are rarely to be found in the wild then the epistemically responsible course of action is to restrict our commitment to the existence and endurance of scientific objects to particularly these and relevantly similar conditions under which success is born out. Here I anticipate the following rejoinder.

My objector would say that since I have already noted that Putnam thinks that what we might call the 'ontology of the conceptual scheme' is not the 'load-bearing' aspect of it then there is not much at stake in the discussion of the endurance of scientific objects. I respond on two fronts

First, Putnam's notion of conceptual scheme is meant to capture the interpenetration of fact and convention where the latter, I said, is understood according to Putnam, as a solution to a coordination problem. Our only access to the underlying facts or states of

affairs is through our representation of them which itself relies on the application of a conceptual scheme. According to Putnam, successful models do faithfully represent. The ontology of their conceptual scheme may not be *the* load-bearing aspect but it certainly is not representationally superfluous. Hence given a successful particular scheme, the objects therein are real, albeit, they are different than ordinary objects. Hence the question of their endurance remains relevant.

Second, since representations – which have conceptual schemes built-in, are perhaps our only way to meaningfully describe the underlying states of affairs, the question of the endurance of the scientific object can be understood as an indirect question about the stability of the underlying states of affairs as represented through a particular representational scheme. The best that we can do to make the claim above in a scheme-independent fashion is to say that the facts, or states affairs – whatever they may be, that are responsible for the success of practice obtain only under very constrained conditions and as such we should not claim that these obtain in outside such and relevantly similar domains. But this claim now not only verges on trivial but also ceases to be a serious form of realism as this is something that even antirealists may be happy to accept. Hence the question about the endurance of scientific object is an important one even on Putnam's account, and what we said makes questionable all forms of realism that take for granted that these objects generally endure.

Consequently, Putnam's convergence realism construed in terms of his tripartite theses of reference, truth and convergence ultimately fail. Direct realism salvages reference to observables but fails to do so for unobservables because our access to the latter is only achieved indirectly by means of representations and this brings back the worry of MTA. Granting that MTA can be overcome – I will say more on that below, Putnam's solution to the problem of incommensurability in the form of CTR also seems to fair well only for observables but not with unobservables. Putnam's principle of charity simply begs the questions against those who do not share his convergence thesis.

What is left of Putnam's realism is his thesis on truth – which presupposes reference, but not convergence. But didn't MTA make reference dubious? Not entirely. I think direct realism does give us a handle on reality and allows us a foot into the unobservable domain. Supplemented by good scientific practices –especially local model building where scientific phenomena are brought under regimented conditions I believe there is good hope to tame MTA. I say tame and not entirely neutralize it because so long as empirical access is representational the threat of MTA cannot be entirely eliminated.

But tame MTA we can. This is especially the case if Putnam is right in that what we are dealing with at the level of unobservables, particularly physics, is an ontology without objects – ordinarily construed. And if the common sense object-based metaphysics which bars inconsistent ontologies is given up, then this invites pluralism of a serious ontological kind. The commitment to such ontology, to be sure, will be restricted to cases of particular

success and the strict conditions brought by our practices therein – particularity and practice will be central themes in chapter 4 and 5.

Notwithstanding MTA, realists have NMA to lean on in support of a viable form of realism, but given our discussion convergence must go, and so does ontological monism – in later chapter we will see that so does universalism. Putnam's phenomena of conceptual pluralism and conceptual relativity were, I think, a step in the right direction. But Putnam, being stuck in a theory-based framework, was unable to entertain other possibilities. In later chapters we will see Cartwright and Chang challenging this theory-based framework thereby opening up new possibilities. Following this discussion, it is helpful to take a step back and consider in what sense what the later Putnam offers is relevant to the future of the scientific realism debate.

3.6. The Relevance of the later-Putnam to the Scientific Realism Debate

In the introduction I explained that, in line with recent developments in the scientific realism debate, I construe the debate broadly to include not only the traditional questions of "is scientific theory T true?" and "does theoretical entity E exist?" but also metaphysical questions such as "what is the world like given our successful theoretical practices?". This last question, the 'metaphysical question' as I called it, is related to the traditional questions but does not reduce to them. This, I explained, is because even if we manage to satisfactorily answer the traditional questions the latter question remains unanswered. For the world contains many things besides theoretical entities. Hence, answering the question of how the world is like given our successful theoretical practices requires in addition that we answer the question of how these theoretical entities relate to other entities. This question I dubbed 'metaphysical' in line with one characterization of metaphysics which takes its task to provide the most general description of reality.

Concerning the traditional questions of truth and reality most of Putnam's contributions have already been absorbed into the realism debate (such as NMA as discussed in section 3.2.1. and CTR as discussed in 3.2.3). Hence these cannot be understood as being contributions to the realism debate in its current form. Yet the later Putnam does provide interesting contributions to the metaphysical question which was either overlooked or underappreciated.

One contribution that Putnam adds to the realism debate is a novel way to solve PMI by providing an account—a metaphysics – of what the world is like that allows that different seemingly incompatible theories to be true at once. As we will see, Chang tries to provide an account of what truth is that allows this but does not then explain what the world could be like that allows this. Put simply, without all the caveats and niceties, Putnam notes that our language is subject/predicate in form. This fits nicely with a world like that envisaged in Wittgenstein's Tractatus, a world of things with features where truth can be taken to be asserting that the right things have the right features. Putnam's world is different – perhaps

not in its entirety rather in certain domains such as the ones described by microphysics. It is a world of states of affairs, as I explained in Section 3.4.1.2. Though this idea may still need more development and defence, it offers a promising metaphysics that can neutralize PMI, particularly that version which targets scientific ontology as discussed in section 3.2.3. The argument, recall, is that if entities which were thought to be responsible for previous scientific success do not exist then this casts doubt on current entities, so long as our commitment to them is based on arguments from success. Putnam's defence of 'ontology without objects' or more precisely his replacement of the common object/property ontology for an ontology of states of affairs in the domain of physics, as discussed in 3.4., helps to solve that.

Whether it is understood diachronically, i.e. concerned with incompatibility over time (e.g. *current* (accepted) oxygen-based models and *old* (discarded) phlogiston-based models), or synchronically, i.e. concerned with incompatibility at the same time (e.g. models of the atomic nucleus), the problem of inconsistent models, I maintain, is the *same*: different successful models representing the same target system make incompatible claims about the system, including positing inconsistent ontology (Morrison 2011).

If we, following the later-Putnam, take our scientific ontology (especially in the domain in physics) to consist of not objects and properties but of states of affairs, this makes it amenable to the phenomenon of conceptual relativity which we discussed in section 3.4.1. And if our ontology is not of objects/properties but of states of affairs, then our faithful representations are also not of objects/properties but of states of affairs, even when the 'surface grammar' or the conceptual scheme of the representation contains objects/properties.

Given an object/property ontology, representing a target system with two representations positing incompatible objects (e.g. phlogiston vs oxygen) and claiming that both are faithful to the same target system is a cardinal sin. For we say there can only be one 'right' ontology, and as such a least one of these is mistaken. But on a states of affairs ontology, this needs not to be. For objects whether oxygen or phlogiston do not *really* belong to the world in any deep metaphysical sense. On the one hand, these objects are artefacts of our representations. On the other hand, given different yet successful practices (and our commitment to argue from success to truth), we would maintain that we managed to represent states of affairs pretty well using different representations positing these incompatible objects. So long as we maintain that our commitment to these is given a certain set of practices within a restricted domain.

It should be noted that Putnam does not succeed in saying a great deal about states of affairs are other than that they are not object/properties and that they are subject to the phenomenon of conceptual relativity. That's why I said above that his work offers a *promise* to help with PMI. Where his contribution lies is in pointing to this possibility and arguing for

its plausibility, or perhaps the necessity, of accepting this new ontology of states of affairs in order to maintain our realist strategy of arguing using success-truth-inference in the face of what seems like cases where our best successful science posits incompatible objects and features as in the case of the particle-wave duality in quantum mechanics.

This much I think was in fact what was intended by Putnam with no real interest to venture into metaphysical speculations. To me, this ontology of states of affairs was predominantly introduced by 'Putnam the realist' to solve the problems that arise from incompatible yet successful representations in quantum physics as explained in section 3.4.1.

Another contribution of Putnam to the metaphysical question of scientific realism is, as discussed in section 3.4.3., to try to answer the enduring question of the relation between science and common sense. As Giuseppina D'Oro notes, claims such as that ordinary common sense objects do not really exist are becoming increasingly common in philosophical context, and *eliminativism* has become a well-established position in analytic philosophy. He adds that, accordingly, the only way to save the manifest objects and properties has been to show that they are 'entailed by' scientific properties (2019, 144).

Against this alienating picture, the later-Putnam provides an answer to the question of the relation between science and common sense thereby contributing to the question of how scientific products are located in the broad scheme of things. The novelty of the later-Putnam's approach is in reversing this picture and convincingly arguing that the scientific realists can, better yet should, accept common sense picture as being equally as fundamental as the scientific picture.

3.7. Conclusion

In this chapter I discussed Putnam's views on scientific realism. A large part of the discussion was on metaphysics and this is no surprise. These two topics are intertwined in Putnam's thought. Having found internal realism unsatisfactory as a scientific *realist* position and having embraced a form of metaphysical realism that sanctions scientific realism, the latter, to the extent that it purports to tell us about the mind-independent reality became, for Putnam, part of his metaphysical realism. I have noted that Putnam's convergent realism is ultimately unsatisfactory. NMA may give support to truth and reference but it simply does not support convergence. As such, we are better off with ditching convergence as a constitutive component of scientific realism.

Also the metaphysical dimension of Putnam's scientific realism includes notable phenomena, such as conceptual relativity and pluralism, the notion of levels of forms, and 'ontology without objects' at least within the domain of physics. Conceptual relativity promises to help us to resolve prima facie inconsistency between different scientific descriptions when these descriptions are 'equivalent', presumably disallowing non-equivalent, i.e. outright inconsistent descriptions. But once we accept an ontology without

objects in certain scientific domains this does allow for a form of serious ontological pluralism that allows prima facie inconsistent ontology, so long as we maintain that what fundamentally exists in such domains are states of affairs and the inconsistency is restricted to the ‘surface grammar’ of our representational schemes, this is while keeping our commitments local, i.e. to particular loci of success and relevantly similar domains. Next I provide a conclusion that ties all three Putnam chapters together.

Putnam's Common Sense Realism: a General Conclusion

The past three chapters aimed expound the later Putnam's common sense realism, which, I said, comprises two components: one on direct realism in perception and one on scientific realism. I divided Putnam on perception into two parts, and I dealt with each in a separate chapter. Chapter 1 dealt with Putnam's defence of what I called the genus direct realism. It ultimately says that we have no reason to give up our native epistemic position of direct realism or what Putnam calls 'the natural realism of common man'. In chapter 2, I explained the later Putnam's defence of his own species of direct realism, which unlike disjunctivistm, has a place for sense data, but no longer grants them the central role they enjoy given indirect realism. In Chapter 3, I explained Putnam's scientific realism, which can be understood as convergence as well as a form of metaphysical realism. His convergent realism, I said, comprises three theses: reference, truth, and convergence. It is committed to reality of 'theoretical entities', to the approximate truth of theories and to theory convergence where by what is meant by that is that earlier theories act as limiting cases of later ones with reference of theoretical terms being preserved across theory-change. He supports it with NMA, and defends it against PMI with his causal theory of reference and his benefit-of-the-doubt principle. I assessed Putnam's convergent realism and his defence of it showing them to be wanting. I explained Putnam's metaphysical realism showing how it can help to solve problems of prima facie inconsistency between prima facie incompatible scientific descriptions but also between scientific and common sense description.

In the final analysis, the question of scientific realism, for the later Putnam, cannot be understood independently of the broader question of metaphysical realism, not least because the answer to the question of how we have access to the mind-independent world which comes up prior to any scientific investigation, has metaphysical implications, namely the commitment to common sense ontology.

Prima facie paradoxically, but ultimately justifiably, rescuing scientific realism requires, for Putnam, endorsing a 'second naivety' about reality and our relation to it, i.e. accepting the legitimacy of the powers that common man long believed himself to have but was later made to doubt by early modern and later by physicalist philosophers, particularly that our semantical notions and abilities are irreducible, and that our perception is direct, the latter being that which gives direct access to the mind-independent world and which purges Kantian and Cartesian scepticism.

At the end of the day, Putnam reminds us that the scientific realist is none other than the common man who should be unphased by the sceptical challenges against direct realism in perception, thereby equally accepting the veracity of his commonsense description of the world as well as his scientific descriptions of it. The later Putnam's common sense realism shows that this much is not only possible but that it is in fact required, and he has led the way in showing us how this can be done.

Chapter 4: Cartwright and Modelling-based Realism

4.1. Introduction

Unlike, for instance, van Fraassen's Constructive Empiricism (1980) or Worrall's Structural Realism (1989), Nancy Cartwright has not laid claim to having a single unified position on scientific realism. She is often understood as defending instrumentalism about laws of nature and scientific theories and realism about entities and capacities.

Overemphasis on some of these particular views has led to the misconstrual of Cartwright's position either as instrumentalist or as entity realist. The latter does seem to be, more or less, the consensus. If one checks the *Stanford Encyclopedia of Philosophy*'s entry on Scientific Realism, one notices that Cartwright is only mentioned once, in a discussion of entity realism (Chakravartty 2011). The same is true of *The Routledge Handbook of Scientific Realism* (2018). I argue that, on closer inspection Cartwright is offering a unified position which I call 'Modelling-based Realism' (*MbR*). The reason, why this has been missed, however, I argue, is because the position that Cartwright is offering fails to fit within the procrustean bed of the theory-based framework within which the realism debate is currently, for the most part, carried out. In this sense Cartwright is offering MbR not only as a position within the debate but also as an overall criticism of the very framework within which the debate has so far been carried out.

In section 4.2., I provide preliminaries that help to flesh out Cartwright's methodological assumptions going into the debate. In section 4.3., I briefly introduce the central arguments in the realism debate. In section 4.4., I explain why Cartwright thinks these arguments are problematic. In section 4.5., I explain Cartwright's criticism of what I call 'Theory-based Realism'. To that end, Cartwright puts forward two arguments: 1- Success of science is not derived from theory, and 2- success of science cannot be derived from theory. I explain these two arguments in section 4.5.1. and section 4.5.2. respectively.

In section 4.6., I explain Cartwright's Model-based alternative. This will require rethinking the status of theory in the new framework. In section 4.6.2., I reject a common reading of Cartwright on theories, namely that they are mere instruments. In section 4.6.3., I explain what theories are given this new framework, namely that they are short-hand labels for powers and our practices for using them. I finish in section 4.6.4. by considering, the status of entities according to Cartwright while contrasting her view with Ian Hacking's entity realism.

MbR is an unconventional form of scientific realism. Particularly it is a model-based particularism, which denies that theories and laws express claims and treats them instead as principles that are short-hand labels for powers and our practices for using them. It accepts as representative, when successful, local system-specific models and is committed to theoretical entities which powers are properties of. It accepts that from these local models

we can construct local claims that are likely to be true of the systems that we have good evidence are correctly represented in those models and of relevantly similar domains without purporting to go beyond them.

Ultimately, MbR as a reconstruction of Cartwright's seemingly diverse views on realism, unlike most other realist views on offer, does not simply pay lip service to the fact *that* the success of science is hard-won. Instead, it explains *how* that hard success is won in practice, thereby helping us to build a responsible scientific image of the world, one which avoids flights of fancy and leaps of faith.

4.2. Methodological Preliminaries

At the outset, it is important to highlight that Cartwright is an empiricist belonging to the Stanford School which emphasizes "pluralism, particularism, and concern with practice" (Cartwright 2019, 91). What follows from the foregoing is a number of methodological injunctions pertaining to Cartwright's argumentative strategies:

1. Rejecting idealisations which make different things seem alike.
2. Making piecemeal, empirically grounded, arguments that avoid great leaps of faith.
3. Shunning "big systems" (for their reliance on long arguments) (91).
4. Constructing our scientific image of the world from our scientific practices that prove successful in interacting with it (29).

4.3. Arguments for and Against Realism

The debate between realists and antirealists has been generally fuelled by two arguments which pull in opposite directions (although see Stanford 2006). These are the No Miracle Argument (NMA) and the Pessimistic Meta-Induction (PMI).

On the one hand, NMA, first introduced by J. J. C. Smart (1963) and later popularized by Hilary Putnam (1979), is a success-based argument that purports to take us from the successes of science to the approximate truth of scientific theories. NMA can be read either as deductive argument (Musgrave 1988) or as an Inference to the Best Explanation (IBE) (Psillos 1999).

On the other hand, PMI, first introduced by Larry Laudan (1981), is a historically motivated argument against realism, which can be read either as an inductive argument to the effect that our current successful scientific theories are likely to be false, or as a reductio ad absurdum that aims to sever the link between success and truthlikeness.

Against the thrust of the antirealist challenge, many realists felt that a 'full-blown scientific realism' is no longer tenable and devised coping strategies that would preserve defensible kinds of realisms. Such strategies include Stathis Psillos's 'divide et impera' (1999, 103), Juha Saatsi's 'success-fuelling properties' (2005, 518) and Anjan Chakravartty's contrast between

'detection properties' and 'auxiliary properties' (2007, 43). All of these are examples of what came to be known as 'selective realism', according to which realists no longer commit themselves to entire theories but parts and/or aspects of theories that are said to do work in securing explanatory or predictive success.

4.4. Cartwright on the Arguments for Realism

Implicit in the foregoing debate is an assumption that is accepted, for the most part, by both realist and antirealists, which is that 'theories' are to be taken as the *units of philosophical analysis*. In other words, when cashing out 'the success of science' in NMA, both proponents and opponents accept that success whether explanatory or predictive, is to be ascribed to scientific theories. The same is true of the different variants of selective realism, all of which start from the same premise, i.e. that a scientific theory is successful, but restrict their commitment not to the theory but to different theoretical posits belonging to it, such as entities, structure, etc.

Emphasis on theories as the units of philosophical analysis and the bearer of scientific success, Cartwright has long argued, goes against how *actual* science achieves the success it does (1995). Most people understood her criticism of the 'theory-dominate' view of science as an attack on realism (see Papineau 1996, 19). Yet few appreciated that Cartwright was attacking *one* form of realism in order to develop *another* more robust form of realism which is informed by scientific practice.

For Cartwright, an antirealist reading of theory is not an attack on realism but is rather the result of a close attention to how the success of science is born out in practice. The conclusion to be drawn from it is not that realism is false but rather that realism cannot be found in theories and should be sought elsewhere. Let us call the framework within which the realism debate has been carried out and which Cartwright opposes "Theory-based Realism" (TbR). Next we consider her arguments against TbR.

4.5. Cartwright against TbR

Cartwright provides two arguments against TbR which vary in strength. The first argument denies that success *is* derived from theories. The second denies that it *can* be derived from principles. I look at each at each in turn.

1. The absence of fixed rules to build models and assess their success.
2. The problem of formulating scientific principles.

4.5.1. On Why Success Is Not Derived from Theory

The first argument builds on an enduring theme in Cartwright's philosophy, featured in (1995), (1999), (Suarez & Cartwright 2008) and most recently (2019): the central role of

models in achieving scientific successes like predicting and , managing the world. Cartwright is not defending the weak claim that models play an important, yet heuristic, role in science. Instead, she is defending the strong claim, which came to be known as *Models as Mediators*, advocated by Morgan *et al*. (1999), according to which models are not generally driven by theories, but instead "play essential roles that are separate from any role they play in constituting theory". That is "models mediate between theory and reality" (2008, 64).

Cartwright's argument is this: If we pay close attention to scientific practice we notice that it is models that provide our scientific successes: our concrete real-world predictions, descriptions, and 'blueprints' for manipulation. These models are almost never constructed by derivation from theories.

Consider for instance the London's brother's model of superconductivity which unlike models before it was able to account not only for the resistanceless flow of current but also the Meissner effect (the expulsion of the magnetic flux when the critical temperature for a material that is transitioning into a superconducting state is reached). Although the model is incompatible with Ohm's law and the resulting acceleration equation, it kept enough of these to maintain at once resistanceless flow and the expulsion of the magnetic field. Suarez and Cartwright explain:

> there was nothing in the theory and the descriptions of different conducting materials that legitimates using Ohm's law for some materials while holding it in abeyance for those that turn out to be superconducting (2008, 65).

Against the syntactic view, Cartwright's claim is that not only is a particular concrete model not deducible from high-level theory, but also that additions, deletions and corrections which warrant the success of that model in that particular context are legitimated by neither the theory itself nor by theoretically acceptable descriptions of the phenomena. In the same vein, against the semantic view, Cartwright's claim is that a representative model (one that is used to represent what the world is like—see later contrast with 'interpretive models') is the result of neither de-idealizations of high-level theory warranted by an acceptable description of the phenomena, nor by the introduction of some different, acceptable, physical assumptions.

The take-home message is that the successes of science in practice are not derived from theory but from system-specific models (2019, 12) – which themselves are very seldom models *of* theory, even the theories that may have been used in their construction. This is already enough to justify Cartwright's dissatisfaction with TbR. However, Cartwright also provides a more recent argument to the effect that not only is success not derived from theory, but also that it *cannot* be derived from theory.

4.5.2. On Why Success Cannot Be Derived from Theory

Cartwright's argument here has perhaps more to do with mathematical sciences where law claims figure, yet remains an important objection against TbR. It is divided into three related components:

1- Many of what we label 'scientific principles' are not formulated properly enough to express propositions.
2- Despite our efforts, we have been unable to turn them into propositions that can do all the jobs we want of them.
3- We do not even use them as propositions.

Cartwright is concerned with the logical form of such principles and whether or not they can be turned into statements that have a truth-value, and can be true and can do the work we want of them. To illustrate her point, she considers, as an example, Stokes's principle and the way it was used in Millikan's oil drop experiment. For an oil drop (sphere) of radius α, Stokes's equation is

Equation 1.1: $F_{drag} = 6\pi\alpha\mu v$.

As things stand, this equation does not express a well-formed formula (wff). Equation 1.2 makes it into one.

Equation 1.2: $F_{drag}(x) = 6\pi\alpha\mu(x)v(x)$

Equation 1.2 is a wff but it is not bounded, so it doesn't express a statement; it has no truth-value. Adding a quantifier turns it into a statement, e.g.:

Equation 1.3:$(x)\,[F_{drag}(x) = 6\pi\alpha\mu(x)v(x)]$

1.3 now expresses a universal statement, which is what we should expect given that Stokes's principle is supposed to express a scientific law.

However, one lesson Cartwright taught in *How the Laws of Physics Lie* (1983) is that scientific laws, what she now calls 'principles', can only hold ceteris paribus. That is, read literally as universally or probabilistically quantified statements, they are strictly false.

Another lesson, from *Dappled World* (1999), is that the astonishing successes of science do not warrant a universal rendering of principles as in 1.3 because these principles, as they stand, generally apply only to "the world as we have made it, not the world as we have found it". That is "our most beautiful and exact applications of the laws of physics are all within the entirely artificial and precisely constrained environment of the modern laboratory" (1999, 46).

In her most recent work (2019), Cartwright puts forward a number of conditions, all of which we would like 'scientific principles' to satisfy but which can't be satisfied together:

1. They express propositions.
2. What these propositions say is in the ballpark of true
3. They have an extensive body of empirical support, which demands that they
    a. Cover most of the well-supported *context-sensitive* claims we take them to govern/describe.
    b. Do not go beyond the domains where they have passed severe tests.
4. They do the work we put them to in prediction and design.
5. They have genuine empirical content (are testable).

Cartwright does not think that these conditions can be all satisfied at once. She notes that the best attempts to turn principles into propositions that express law claims and satisfy the conditions above ultimately fail. One attempt, Cartwright explains, follows the strategy in Equations 1.1 – 1.3. For example, Newton's second law becomes:

$$L = (x)[F(x) = m(x)a(x)]$$

L is a proper proposition but Cartwright maintains that it is not true. We may get a truth –L'- she argues, if we attach a "nothing-else rider" (NE):

> So long as nothing operates to affect the consequence described in L except things that can be represented with the concepts in the antecedent of L, then L' (2019, 22).

Cartwright explains that L', which is just L protected from "finks, antidotes, masks, defeaters, interferences, and the like" by attaching NE to it (2019, 32), can satisfy conditions 1, 2 and 3 but not without violating 4 (22). Her argument here is intricate, and getting caught up in its details will derail us from the aim is chapter. I offer a brief incomplete rendering to bring the point home.

For Cartwright "the general exists only in the particular" (1999, 38). As such, warrant for an *abstract* principle is rooted in success born out in *concrete* situations. Thus, abstract principles need "fitting out" into more concrete descriptions in order to have truth-value (40). Otherwise they are neither true nor false. They are, as per Pierre Duhem mere "symbolic representations" (2019, 6). Fitting out these abstract concepts in more concrete forms in a way that is theoretically warranted requires the use of *interpretative models* (in the syntactic mode – 'bridge principles') that are licensed by the theory. These models can be applied to concrete systems and their success give support to the propositionalized principle, thus already satisfying conditions 1,2 , 3a, and 3b.

However, Cartwright argues, there just aren't enough of them. As previously suggested, most real world situations contain factors for which there are no interpretive models in the theory but which must be accounted for if the model is to represent the world accurately enough to make correct predictions and allow successful designs. That is why we must build

more concrete details into the models we use to represent the world (our *representative* models) in order to achieve success. She notes that Millikan himself did not use Stokes's principle in its textbook form. Instead, for it to do the work he wanted of it, he had to add corrections due to "undetermined factors" which had no theoretical basis (2019, 15).

The problem that abstract theorical principles face even when they are turned into propositions via interpretative models is that they fail to do the work we put them to in prediction and design outside very limited domains – hence violating 4, because, unlike representative models that do not literally exemplify the theoretical principles, they are insensitive to variation in context.

In a nutshell, Cartwright's argument is this: to the extent that an equation can be formulated as a proper universal (or probabilistic) claim, it is either false (as with L) or, as with L', fails to yield success in prediction and design in domains which are relevantly different from those tidy ones in which all relevant factors attach to theorical descriptions via interpretative models. And to the extent that we can formulate a variant of L' such as L'_n which can be successfully used in designing real-world systems, each context-dependent L'_n cannot be universal. For a principle, being useful in real-world systems and being true and universal simply pull in opposite directions. Cartwright concludes that success cannot be derived from principles.

So far we have seen why Cartwright thinks that TbR is mistaken. Theories are predominantly not the bearers of success (2019, 7). That is why her criticism of theory and principles should be understood as arising *not* from within TbR but from without. This subtle, yet seldom noticed, point has important implications. That is because, had Cartwright made her criticism from within TbR, her position may rightly be recognized as instrumentalist. However, that Cartwright is keen on distancing herself from instrumentalism or any form of anti-realism for that matter despite having made what prima facie looks like an instrumentalist move means that there is more to this than simply choosing sides – we will see later why an instrumentalist reading of Cartwright is mistaken (20).

In truth, Cartwright is criticizing a framework. The thrust of her argument is that both proponents and opponents of realism who subscribe to TbR are barking up the wrong tree. Consequently, we need to provide an alternative framework where emphasis is no longer placed on theories but rather on that which secures success in practice and this is essentially what Cartwright offers.

4.6. Modelling-based Realism

4.6.1. A Practice-based Framework.

Prima facie, it might seem, having given up on theory, that the right alternative is 'model'. But that would be too quick an answer, because by now, it should be clear that no single

model can represent all target-systems. In fact, Cartwright's introduction of the representative models against interpretative models was meant to solve that very problem.

In the alternative framework Cartwright sought and found in scientific practice, science is best understood not as a *product*, but rather predominantly as a *process* that bears products (see Soler et al, 2014). TbR's mistake is its fixation on product to the detriment of process. Once that is corrected, we can start to look at science as a set of practices or as epistemic activities, as defended by Hasok Chang (2014). Chang's characterization of epistemic activities, however, is made in terms of operations (67), following his rehabilitation of operationalism (Chang 2017). But Cartwright sees difficulties in opertationalism, which according to her "belies experimental practice" (2000, 243).

Alternatively, Cartwright's takes 'modelling' as her activity of choice to capture successful scientific practice, hence my characterization of her position as 'Model*ling*-based Realism" (MbR) rather than "Model-based Realism".

A forceful expression of MbR is provided at the outset of Cartwright (2019) where she writes:

> Central thesis: *techné* provides the very best representations of Nature that are possible, human or otherwise – because this is just what Nature is like (4).

Cartwright explains that this thesis is neither pragmatic, i.e. serving mere practical purposes, nor epistemic i.e. reflecting the limits of our understanding. Rather, it is ontological. Nature is, in fact, exactly the way it is represented by successful models.

Central to MbR is the rejection of the distinction made by Aristotle between episteme and techné, at least in so far as episteme is taken to denote *genuine* knowledge while techné is taken to denote "mere" art and craft. Consequently, Cartwright holds that "techné can embody genuine knowledge" (5). What is meant by techné is 'artful modelling', which in the absence of fixed rules of derivation is not only how we do achieve our very best representations of Nature but the only means by which we can do so. The art here lies in 1- knowing what pieces of knowledge to call on 2- understanding what can be done with them and 3- *arranging* these different kinds of knowledge to build context-specific models that lead to accurate predictions (4).

4.6.2. The Status of Theory in MbR

So far I have explained that according to MbR, techné can embody genuine knowledge and that successful techné-based representative models provide the best representations of nature within their corresponding domains. What led to this shift to MbR, I explained, was Cartwright's realization that success in practice is poorly accounted for within TbR. But an important question still stands regarding the status of theories in MbR.

Cartwright's criticism above showed what theoretical principles *are not*. Her arguments if successful, establish that:

1- They are not universal claims.
2- They are not even claims.
3- Turned into claims, they are almost always false or fail to account for success in practice.

What remains to be explained is what *they are* given MbR. A popular answer which was defended earlier by Cartwright (1995) and which has stuck with her ever since (see French & Saatsi, 2014) is that they are instruments, or in Cartwright, Shomar, and Suarez's words, tools in the 'toolbox of science' (1995, 138). That is the reason why Cartwright has been dubbed an instrumentalist. This may even be thought to be confirmed by her most recent claim that theoretical principles are symbolic representations (2019, 6). A closer look, however, reveals that this is far from the whole picture.

Instrumentalism, in science, is the view that scientific concepts, theories, and cognitive entities are to be treated as tools and instruments to save the phenomena (Stanford, 2005). Accordingly, scientific theories are not assertoric. In the words of Gilbert Ryle (1949, 117), they are 'inference tickets' allowing us to infer from one observable state to another. On such a reading, so long as a phenomenon is saved or inference is made, the form of the tool or the ticket does not matter. It does not figure in the end product. Put plainly, the hammer you use to build a wall is not part of the wall.

But that is not what we see theories do in MbR. We can clearly see, to take Cartwright's example, that the equation Millikan used in his oil drop experiment, which is $F_{drag} = 6\pi\alpha\mu v/(1 + Al/\alpha)$, very much resembles Stokes's principle ($F_{drag} = 6\pi\alpha\mu v$), although with the necessary amendments. So it is not the case that Stokes's principle was simply a tool. To use a chemical analogy, Stokes's principle is not a catalyst which helps drive the chemical reaction while ultimately not undergoing any chemical change. It is rather a reagent in that reaction, which, although transformed, still somewhat figures in the product. But if theories are more akin to building blocks than to tools then what are they?

4.6.3. Theories, Powers and Nomological Machines

According to Cartwright, theories are not only symbolic representations but also, and more importantly, they are "short-hand labels for powers and our practices for using them" (2019, ix). The reason why powers are introduced is that, according to Cartwright, we are unable to account for why regularities hold in some special situations, rather than others, while sticking to an ontology "without modality" (2019, 31). A trias power ontology is needed to make sense of our most successful methods we apply in practice, amongst which is the 'analytic method' (29).

> "The analytic method is two-stage. First, identify how powers exercise when they operate 'on their own' […] Second, derive what the overall results will be in a given situation by the use of some rule for calculating what happens when the powers that operate in that situation are exercised together" (2017, 2).

Crucial in this characterization is the emphasis on *exercising*. That is because on the trias power ontology, powers not only *obtain*, which they do, but they also *exercise* in the sense that they contribute to the *obtaining of the outcome*. It is noteworthy that the exercising of the power may in fact be of an entirely different kind than that of the occurring outcome. This explains why Cartwright's trias power ontology is actually a *trias* ontology, for it comprises:

1. The obtaining of a power.
2. Its exercising.
3. The obtaining of the outcome.

The often mentioned 'special situations' which are artfully modelled so that powers exercise in concert leading to stable regularities are Cartwright's nomological machines (1999). A more updated characterization is the following:

> A nomological machine is an arrangement of powers that, if set working repeatedly, would give rise to the kinds of patterns of happenings that 'laws of nature' are typically thought to describe (2019, 29)

To understand how powers function given a nomological machine we should keep note of 1- their goal-directedness, 2- their constraints, 3- the role of arrangement.

What is meant by goal-directedness is that powers point towards a certain outcome. For instance, "the goal of gravity is to produce a force $F_g = GMm/r^2$" even if it does not end up producing that (2019, 36). As for constraints, a certain power can only influence one *kind* of outcome. In our example, gravity can influence the outcome brought about by a nomological machine only by influencing *force*. Yet despite this rigidity, one can through techné construct all sorts of nomological machines which produce all sorts of outcomes through clever arrangement of powers, which is the ultimate source of permissiveness. In fact, Cartwright contends that despite the aforementioned constraints, "clever arrangements allow a power to participate in producing *almost any outcome*" (2019, 36, emphasis added).

4.6.4. The Status of Entities in MbR

MbR starts from Cartwright's empiricist dictum which urges that we construct our scientific image of the world from our scientific practices that prove successful in interacting with it (2019, 29). It is grounded methodologically in techné and ontologically in powers. Mutatis mutandis, powers are themselves properties. Hence, if we accept them as real, which we often do, then we must accept that features which have them are also real. That is why we

see Cartwright readily accepting the reality of theoretical entities like electrons, DNA, or the democratic peace (20).

Commentators who only paid attention to Cartwright's conclusion that some theoretical entities are real – and not the way she arrived at it, are often to lead to equate her position with Ian Hacking's entity realist realism (1983). I give three differences that show that Cartwright's MbR is not Hacking's entity realism.

1. Methodology: Cartwright is committed to theoretical entities that feature in causal explanations – where this causal explanation is supported by rigorous experimental testing (1983, 98). Hacking, on the other hand, limits his commitments to theoretical entities that are warranted by *only a subset* of causal explanations, namely that which involve the *manipulation* of the entities in question. Hacking writes that "experimenting on an entity does not commit you to believing that it exists. Only manipulating an entity in order to experiment on something else need do that" (1983, 263).
2. Ontology: Hacking is known for his claim that "if you can spray them then they are real" (23). But Cartwright advises a more cautious claim which is that "*when* you can spray them, they are real" (1999, 34). The difference between Hacking's 'if'and Cartwright's 'when' is immense and pertains to the *scope* of each authors' ontological commitment. On the one hand, Hacking is happy to extend his commitment to theoretical entities well-beyond their experimental context, thereby advocating a *global* ontology. On the other hand, Cartwright, given her empiricist dictum, is more cautious about her ontological commitments, restricting them to tried and trusted domains which are none other than nomological machines, thereby advocating a *local* ontology.
3. The place theory: entity realism has been charged with incoherence for its claim to be theory-independent (Resnik 1994). It also has been recently argued that any attempt to make the position coherent collapses it into standard scientific realism (Massimi 2004). Ironically, despite its rejection of theory, entity realism still operates within TbR, hence the source of incoherence. Conversely, MbR faces no such difficulty. It has a place for both entities and theories, although theories now are no longer understood as propositions but rather as symbolic representations designating powers and our practices of using them.

4.6.5. Psillos's Challenge and a Response

Cartwright's rejection of universal laws and theories in favour of entities and powers has elicited strong responses from conventional realists who were keen to defend laws against Cartwright's attack. A notable response comes from Stathis Psillos (2009).

Psillos acknowledges the attractiveness of Cartwright's power-ontology but also presents an important challenge against it, namely to provide a way to identify powers. This challenge, Psillos argues, leads Cartwright into the following dilemma (100).

On the hand, powers are properties and properties may be identified by the laws that they participate in. This meets the challenge, i.e. helps to identify powers, however, at the expense of countenancing laws which powers, Psillos claims, were introduced to replace in the first place. On the other hand, powers can be identified by a set of behaviours that occur when they manifest – Psillos's word. But this is not very helpful since Psillos argues that Cartwright holds that powers can manifest in almost any behaviour. How would Cartwright respond to this challenge?

On the face of it, it seems that Psillos thinks that Cartwright is not happy to countenance *any* laws, but this does not seem right to me. My previous discussion of Cartwright on 'principles' shows that her argument is against a very precise construal of laws, namely one which express propositions that are both universal in scope and are able to do the work we want of them in prediction and design. I have also explained that Cartwright claims that we can manage to turn some principles into law claims, but I have also noted that these would not be universal. Hence, it is a mistaken, I believe, on Psillos's part to deny Cartwright's recourse to laws tout court.

As such, one way to meet Psillos's challenge may be to rely on more local laws to identify powers. Cartwright even seems to be going down that line when she claims that powers may be identifiable by reference to laws which, in turn, she claims often involve reference to other powers as well. An example of laws that she endorses is "An object of mass *m* has a capacity of strength $GMm/r2$ to attract a mass of size *M* a distance *r* away" (Cartwright 2008, 195). This already solves the issue of providing means to identify powers, but the other horn of the dilemma reflects a misunderstanding of Psillos's of how Cartwright's power ontology works.

Psillos worries, given that, for Cartwright, powers can manifest in almost any behaviour, that this would make it very difficult to identify them by their manifestations (2009, 122). It is helpful to here to go back to Cartwright's own terminology, namely characterizing, powers in terms of 1-their obtaining, 2- their exercising and 3-the obtaining of the outcome. I take it that what Psillos means by manifestation is the obtaining the power or of its outcome since there is no indication that he acknowledges her tri-partite ontology. If that is the case then Psillos's worry is misplaced. Again consider the gravitational power. This seems to always obtain, in that massive objects attract each other. But its canonical behaviour of acceleration may seldom occur. Yet Cartwright notes that we have a host of tests that we can perform that assure the obtaining of powers even when their canonical behaviour does not occur (2008, 195). Importantly, we need not worry that the powers' obtaining can result in almost any outcome might hinder our ability to identify them, so long as there are precise rules that explain how powers combine so that the fact that they exercised as they are

supposed to can sometimes be vouchsafed. So, we can suppose that powers can be individuated by their canonical exercisings and we can be sure that a particular power exists in the world by backreading that an exercising of the appropriate kind has occurred from manifestations (in the sense of outcomes) that are observed to occur. Also, in many cases we are able to construct experimental conditions in which a single power is responsible for the outcome so we can directly read off the exercising from the observed outcome. Both of these depend on the assumption that we are right about the other powers in those settings and how they exercise. But this is standard in science: we rely on previously established knowledge to bootstrap to new.

4.7. The Relevance of Cartwright to the Scientific Realism Debate

After this reconstruction of Cartwright's views on realism, and in line with the aim of this dissertation to expound unconventional views on realism that promise to take the debate forward, it is relevant to ask what does MbR offer over and above what Cartwright's already known views do? Why couldn't we just accept that Cartwright is an instrumentalist about laws and theories and a realist about entities and powers? Don't these views broadly capture Cartwright on realism without having to worry whether MbR is really underlying them? Answering these questions, I say that indeed Cartwright is an instrumentalist about laws and theories and realist about entities and powers. But as my reconstruction has hopefully shown, these are but the results of Cartwright's position and neither of them is *the* position that Cartwright defends in the realism debate. For these follow from MbR and are not themselves independent of it. That is why I said that overemphasis on either (instrumentalism or entity realism) has led to people to overlook Cartwright's major methodological contribution to the realism debate, namely her rejection of the current theory-based framework in the realism debate for another model-based alternative. This is relevant for two reasons.

First, and this is no minor point, if Cartwright is indeed rejecting the framework wherein the current realism debate is being carried out, as I argue she is, for another alternative then as a matter of good scholarship we had better do her justice and try to faithfully represent her views as best we can. Second, and this is pertinent to the current realism debate, Cartwright's change of framework does have important implications to our realistic commitments which current forms of realism fail to appreciate. I show that in what follows.

In defending success-to-truth inferences against historical challenges such as PMI, scientific realists have devised two, not incompatible, coping strategies, one of which I already touched on, namely to be *selective* realists, and the other is to go local.

1. Selective Realism: we need not commit to entire theories. Instead we, 'select' parts and/or aspects of theories that do work in securing explanatory or predictive success (Vickers 2017).

2. Localism: we need not hold a single view on the (approximate) truth of *all* contemporary, well-confirmed, theories, or the existence of all the unobservable entities postulated by those theories. Instead, we can pick and choose (Asay 2019).

Selective realists may disagree amongst themselves on the 'unit of commitment', i.e. the 'working posit' to which we should commit (e.g. entities, structure, etc.). They, nonetheless, generally agree with localists and traditional realists that the 'unit of analysis' is theory. The distinction between 'unit of analysis' and 'unit of commitment' helps to show why these strategies are misguided – I expand on that further in the conclusion.

The viability of the realists' success-to-truth argument stands or falls with choosing the right unit of analysis, i.e. the proper bearer of success. This is crucial because the choice of unit of analysis affects the appropriate unit of commitment (e.g. choosing universal theories implies that our commitments should be universal in scope). Yet, practice-oriented philosophers of science, at the forefront of whom is Cartwright, have long urged that the proper bearer of scientific success is not 'theory'. How does that help to advance the scientific realism debate?

The answer to that lies in that Cartwright construed as a modelling-based realist is an invitation for players in the realism debate to be wary of the scope of their realistic commitment. That is if local models are indeed the units of analysis then our realistic commitments should equally be local, be they about entities, structure or what have you.

4.8. Conclusion

MbR provides at once a criticism of the framework within which the scientific realism debate is being currently carried out, as well as a corrective. It claims that science's theoretical success is not derived from theories but instead is grounded in practices which themselves rely on techné. It accepts as representative, when successful, local system-specific models and is committed to theoretical entities which powers are properties of. It accepts local claims that are constructed from these successful local system-specific models and takes these to be likely to be true of those systems that we have evidence that the models successfully represent and relevantly similar domains. It dethrones theories without turning them into mere instruments thus counting as neither instrumentalism nor entity realism and stands as an important candidate in the scientific realism debate.

Chapter 5: Probing 'Operational Coherence' in Hasok Chang's Pragmatic Realism

5.1. Introduction

Hasok Chang is developing a new form of scientific realism that he calls 'pragmatic scientific realism'. He aims to reorient the debate away from truth and towards practice. Central to his project is replacing truth as correspondence with his new notion of 'operational coherence', which is introduced as:

1) A success term with probative value to judge and guide epistemic activities.
2) A more useful alternative than truth as correspondence in guiding scientific practice.

I argue that, given its current construal as neither necessary nor sufficient for success, operational coherence is too weak and fails to satisfy both 1) and 2). I offer a stronger construal of operational coherence which aims to improve on Chang's account by tying it to systematic success thereby making it both necessary and sufficient for success. This new account, if successful, rescues 1) but not 2). This chapter is divided into two parts.

In the first part, I introduce Chang's pragmatic realism. I explain the motivations underlying his reorientation from truth to practice, which Chang does by replacing the current proposition-based framework with a practice-based alternative. The latter is cashed out in terms of 'epistemic activities' and 'systems of practice' and I explain these in turn. 'Reality' and 'truth' take on new meanings given this new framework. I explain these new notions. The upshot of Chang's position is a scientific pluralism according to which prima facie contradictory theories can be true.

In the second part, I borrow the notion of 'probative value' from legal context where it is used to denote the power of a piece of evidence to support an alleged proposition and use it in the context of philosophy of science to denote the power of whatever it is a power of to support that we are minimally 'getting things right' in the world. I use probative value to assess 'operational coherence'. I argue that operational coherence, construed by Chang as neither necessary nor sufficient for success, lacks the probative value that correspondence truth has, thereby failing to satisfy 1). Truth as correspondence, I maintain, even if it is construed in a liberal sense that allows for pluralism, wears its probative value on its sleeve in virtue of being a correspondence relation between propositions and the mind-independent world. I also show that the same route taken to defend the usefulness of operational coherence in guiding practice is available for proponents of correspondence. Operational coherence thereby fails to satisfy 2). I offer a stronger notion of coherence which ties it to systematic success. This rescues the probative value of coherence while leaving the question of its superiority over correspondence in practice open.

Then, I take step back and try to locate Chang's pragmatic realism within the bigger philosophical picture by taking on two questions. First, in what sense is Chang's pragmatic realism pragmatic? I answer that by comparing his views to the founding fathers of pragmatism, Pierce, James and Dewey. Second, in what sense, if at all, is it a form of realism? I answer that Chang's position is a form of deep perspectival realism, and I defend it against the charge of relativism.

In a postscript I provide some remarks comments on the epistemology of science pointing out that, although no current philosophy of science offers both, there is nothing intrinsic about a practice-based philosophy of science that precludes having operational coherence as well correspondence. I argue that given a proper understanding these two notions could, in fact, be understood as complementary. I suggest one way this could be done.

5.2. New Framework

Chang wants to take the scientific realism debate beyond foot-stamping. He wants scientific realism to be relevant and useful to scientific practice. That is why he finds construing scientific realism merely as a descriptive thesis about the aims of science to be underwhelming and suggests a more 'active' alternative. In this sense, Chang takes realism to be a policy, better yet, an ideology that commit us to maximize our learning from reality (2018, 31). Given his pragmatic slant, Chang tends to blur the line between knowing and doing. As such, maximizing our learning from reality, for him, can be achieved by maximizing our successful doing in reality. It is for this reason that he is interested in reviving "useful ideas and facts lost in the record of past science" (2004, 237). For these can help us do things we cannot do with our current science.

To that end, Chang holds that we need a framework that would help us to make sense of successful scientific practice past and present. But he hastens to add that the current *proposition-based framework* is not fit for purpose (Chang, 2017a). For Chang, Kuhn has shown that at crucial moments in the history of science, scientists were choosing not between theories but between entire paradigms (Kuhn, 2012). Practice-oriented philosophers of science have convincingly argued that emphasis on theories, understood as bodies of propositions, leads to a distorted analysis of science as it neglects all the non-propositional aspects such as experimentation and other non-verbal activities which are constitutive of science (Chang, 2014). As a remedy, Chang proposes an *action-based framework* in terms of "epistemic activity" (*EA*) and "system of practice" (*SP*).

5.2.1. Epistemic Activities:

Chang characterizes an EA as:

> a more-or-less coherent set of mental or physical operations that are intended to contribute to the production or improvement of knowledge in a particular

> way, in accordance with some discernible rules (though the rules may be unarticulated) (2012, 15).

An EA is then:

- A coherent set of operations.
- Abstractly characterized but concretely realized.
- Partly defined by aims. (This distinguishes them from 'mere physical happenings' involving bodily movement, e.g. purposelessly moving one's arms.)

Chang uses 'epistemic' broadly to mean 'in relation to knowledge' without tying this notion to the notion of truth in strict realist sense (2009, 80). This he does by taking that kind of knowledge that EA is concerned with to be non-propositional, i.e. know-*how* instead of know-*that* (2012, 215). Examples of EA include measurements, detections, DNA extraction, and also simpler and more day-to-day activities such as match-lighting.

In the context of a SP, an EA has at least two kinds of epistemic aims – although it may have other kinds of aims (e.g. sociological aims):

1. "Inherent purpose" which is the aim partly defining the EA
2. "External function" which unite different EAs under a SP.

5.2.2. Systems of Practice

A SP is characterized by Chang as:

> [a] coherent set of epistemic activities performed with a view to achieve certain aims. . . . Similarly as with the coherence of each activity, it is the overall aims of a system of practice that define what it means for the system to be coherent (2012, 16).

A SP is then:

- A coherent set of EAs
- Whose coherence is partly defined by the *overall* aims.

The following example helps to illustrate how EAs and their aims come together under the rubric of SP and its overall aims:

Consider the EA of DNA extraction. It is a procedure of isolating DNA from the nuclei of cells. If we ask "what is the aim of this activity?" a straightforward answer would be to extract DNA from nuclei of cells. This is the *inherent purpose* of the DNA extraction. But we can extract DNA for all sorts of purposes, such as to determine the characteristics of a certain gene including its structure and function, or to purify it and use it in gene-based vaccination, or to use it to transfect other cells in experimental contexts, etc., and we cannot, by simply observing someone performing a DNA extraction, decide what that person intends to do with the extracted DNA. That is because there is nothing intrinsic about DNA extraction

which says what the extracted DNA is going to be used for. The latter aim is called the *external function* (of DNA extraction). It is determined, according to Chang, by the role the EA plays along with other EAs in achieving the overall aims of the SP in which it is imbedded. So if we take, for instance, the overall aim of the SP to be to perform gene-therapy, then the external function of DNA extraction would be to develop a gene-based vaccine.

Against this neat picture it is important to note a certain ambiguity in Chang's notion of SP. On the one hand we have the clear characterization of what a SP is laid out above. It tells us that a SP is characterized by a set of EAs and overall aims. But elsewhere we find Chang noting that his SP is similar to Kuhn's 'paradigm', where the latter is understood as 'disciplinary matrix' that includes "all kinds of elements ranging from fundamental metaphysical principles to institutional structures" (2012, 18).

Chang notes that Kuhn's notion of paradigm qua disciplinary matrix is similar to what he had in mind when he was developing his new framework but decided not to use it and introduced his own terminology of SP for two reasons. One is that he finds that 'paradigm' is tightly linked to Kuhn's view of scientific development which Chang does not share. Two, Kuhn does not provide a clear indication of how the different elements in a 'disciplinary matrix' are held together, whereas the notion of SP is 'more definite and are more orderly'.

The Ambiguity here lies in the fact that elements such as 'metaphysical principles' and 'institutional structures' which Chang takes to belong to SP seem to have no place on Chang's first characterization SP in terms of EAs and overall aims. Hence it seems that Chang is using SP in at least two different senses a *narrow* sense as characterized above and a *broad* '*Kuhnian' sense which captures the narrow sense and includes all the other elements that belong to Kuhn's disciplinary matrix. For my own purposes when I use SP I the discussions below, I mean it in the broader.* Having introduced the broad framework I now consider the new meaning that Chang gives to 'reality' and 'truth'.

5.2.3. Pragmatic Reality

Chang distinguishes *uppercase R* Reality and *lowercase r* reality. By 'Reality' he means the mind-independent reality. About it, he holds, similarly to Kant, "we can and should say nothing" (2018, 32). Sparing Chang a contradiction, we can say that Chang thinks that mind-independent reality is in a sense *thinkable*, i.e. we can affirm its existence even if we are unable to meaningfully talk about it. This is why he says that we cannot learn *about* Reality but can only learn *from* it (Chang 2017b, 181).

By 'reality', which he also calls 'real-ness', he offers conditions which when satisfied entitles entities involved in EAs to deserve our realistic confidence – this is further explained below. Chang arrives at this new notion starting from Ian Hacking's (1983) 'experimental realism', which takes the manipulation of an entity to give us warrant for taking it as real. Chang

transforms Hacking's criterion into a generalized theory of what entities should be counted as 'real', which he calls the *coherence theory of reality* according to which:

> *A putative entity should be considered real if it is employed in a coherent epistemic activity that relies on its existence and its basic properties (by which we identify it)* (2016, 116).

By 'relies' Chang is invoking necessity, a 'pragmatic necessity', which he describes elsewhere as a "necessity arising from the requirements of action" (2009, 70), and this necessity we can only learn about empirically (2017a). Accordingly, if accepting the existence of a postulated entity is pragmatically necessary for performing a coherent and successful EA, then we should take that entity as real.

In *Is Water H2O?* (2012) Chang provides a detailed study of 'phlogiston' which is characterized as "the principle that imparted combustibility to combustibles" (3). Phlogiston is considered not real by current light. Yet, given a phlogiston-based SP, Chang argues, Joseph Priestly and others chemists of the time were able to carry out numerous successful experiments whose coherence depends on their taking that there is an entity which is phlogiston which they are able to manipulate. One such experiment, for instance, is the revivification of mercury calx into its metallic form by allowing it to absorb the phlogiston from the air. This EA of de-phlogisticating air by exposing it to mercury calx has as inherent purpose the 'de-phlogistication' of air. The coherence of this EA would have been impossible without taking that there is phlogiston (4-5). Chang's argument for the reality of Phlogiston can be summarized as:

1. Phlogiston being taken as real is necessary for the coherence of a bunch of successful EAs.
2. Success is our best indicator of truth. Therefore, phlogiston is real. This is while keeping in mind that we are not to talk about (capital R) Reality but only what should count as real, i.e. (lowercase r) reality. So phlogiston should count as real –which for Chang is 'really real' in the only sense we can talk about.

5.2.4. Pragmatic Truth

'Truth as correspondence' broadly characterizes truth as a relational property that a proposition has with a relevant portion of reality such as a fact, state of affairs, etc. It says that truth obtains when there is a correspondence, congruence, agreement, etc. between that proposition and that aspect of reality. Chang thinks that truth as correspondence is not useful in guiding practice but he does not entirely relinquish the notion of truth. Instead, he gives it a place subordinate to coherence in his framework. He characterizes truth as:

> A statement is true in a given circumstance if (belief in) it is needed in a coherent activity (2017a, 113).

Similarly to the case of reality, 'needed' above is also meant to convey 'pragmatic necessity' i.e. that belief in or assent to a statement is required for carrying out a coherent and successful EA. So in our DNA extraction example above, the statement that "cells contain DNA" is true—in Chang's sense of pragmatic truth-- because it is necessary for the activity of DNA extraction to be coherent, so is the statement that "air contains phlogiston" in order to de-phlogisticate air. What our discussion so far reveals is that, according to Chang, entities and statements belonging to successful EAs can be real and true respectively. Yet, this raises the following important issue. At certain points in history we may have competing SPs, each postulating different entities each successful at its related aims but not likely all to be real at once. How does that affect the metaphysical picture of the world that science, given pragmatic realism, gives us?

5.3. The Case for Metaphysical Pluralism

Chang's response to the former question is that we should be 'metaphysical pluralist' – although a better wording would have been 'ontological pluralism' as he is concerned with having a multiplicity of objects or entities. Be that as it may, once we accept Chang's new practice-based framework along with his theories of pragmatic truth and reality, it follows that we should countenance a form of 'practice-based metaphysical pluralism' according to which we accept as real, in the pragmatic sense, entities that are pragmatically necessary for carrying out coherent and successful EAs.

But Chang thinks that we arrive at metaphysical pluralism even if we do not subscribe to his pragmatic realism. That is even if we were scientific realists in the traditional sense, Chang thinks, we would also end up in metaphysical pluralism so long as we pays close attention to 'success' in success-to-truth inferences. This conclusion, Chang holds, can be arrived at in a three-step fashion:

1) Accept the traditional realist success-to-truth argument, famously known as the No Miracle Argument (NMA).
2) Show that the success of science is multi-dimensional.
3) Defend that plurality of success leads to a metaphysical pluralism (2017b, 176)

Given that Chang is addressing scientific realists, most of whom already accept some form of NMA, he simply accepts the validity of the success-to-truth arguments. He notes, however, that 'success' in NMA is often used without precise characterization. Chang explains that success, understood in its broadest sense, is the achievement of aims of relevant agents who may have different, yet equally legitimate, epistemic aims. So we already have one kind of pluralism entering at the level of aims. These aims, however, Chang notes, are partially definable by the many epistemic values that we hold (177). But perhaps 'definable' is too strong, as aims are not always defined by values unless the person having certain aims intends for them to be so, such as "to explain *X* according to the *simplest* theory available", otherwise an aim can be simply "to explain *X*, according to any of

the theories available". But, as will be discussed below, aims are intentional and given that we are not always aware of the values we subscribe to we cannot say that aims, epistemic or otherwise, are 'definable' by values.

This is not really a problem though as we can accept a more modest claim, namely that aims are always affected by or qualified with values. Such qualification needs not to be intentional and we need not even be aware of the values at play. One may wish to explain a certain state of affairs *X* using a particular theory, which is simpler than other theories, although she was not aiming for or even thinking about simplicity. Here 'simplicity' qua an epistemic value of a theory affects or qualifies the aim of the explanatory activity.

With these minor qualifications in place, we continue with Chang's argument which says that epistemic values – which we now say qualify or affect agents' aims, are various and they are thought by realists to be truth-conducive. This opens the door for different criteria of success and as such for different success-to-truth arguments, leading eventually to metaphysical pluralism. A pertinent example of the kinds of metaphysical pluralism that ensues from accepting success-to-truth arguments is having to accept as representative multiple successful models that make fundamentally incompatible assumptions about the underlying states of affairs (e.g. models of the atomic nucleus, see Morrison (2015)).

This pluralism, Chang notes, is inescapable even if we were value monists and took for instance, empirical adequacy as the sole value of science and decided to infer from empirical adequacy to truth. That is because, Chang explains, in practice there is no 'perfect empirical adequacy'. Empirical success, he notes, is multi-dimensional and Kuhn's (1977, 322) famous list of accuracy, consistency, scope, simplicity, and fruitfulness, which often pull in opposite directions, are best understood as capturing different dimensions of empirical success. Also here pluralism ensues (Chang 2017b, 177).

Having accepted I. and II., what stands in the face of metaphysical pluralism, Chang explains, is a deep-seated belief, held by traditional realists, in metaphysical monism according to which the world comprises one fixed set of objects with one fixed set of properties. Chang finds this belief unsupported. He adds that if we accept success-to-truth arguments and realize that success is multidimensional then metaphysical pluralism is inescapable. What follows from metaphysical pluralism is that mutually contradictory theories can both be true (179).

Recall the case for the reality of phlogiston: its being pragmatically necessary for the coherence of successful EAs. Chang notes that there is nothing special about phlogiston that makes it an anomaly in the history of science. Indeed, a host of other entities that are abandoned by current science such as caloric, 'frigorific' radiation, etc. have also been pragmatically necessary for the coherence of a host of other successful EAs given different SPs. He adds, that the same Hacking-type inference can be made for those as well, leading us to conclude that these are also real.

The existence of such cases in history is not news for philosophers engaged in the realism debate. After all, the claim that terms such as 'phlogiston' and 'caloric', which were central to successful but discarded theories, are non-referring is what feeds Larry Laudan's now notorious pessimistic meta-induction (PMI) (1981). However, what's new about Chang's position is that it shines new light on PMI, showing that understanding PMI as an argument against realism is premised on the hidden assumption of metaphysical monism. Once monism is given up, PMI is instantly transformed into a positive argument for metaphysical pluralism. What follows is that phlogiston and caloric can be equally as real as electrons and positrons (Chang 2017b, 181).

It is worth mentioning at this point that many more traditional realists may find Chang's claims unconvincing not because they disagree with him on the claim that there are different criteria of success. They don't. But because they hold that these different criteria give the realist room to qualify the kind of success that they consider to warrant a success-to-truth inference (see Vickers 2019). The challenge this approach faces is in being able to provide criteria which answer the historical challenge in a way that is not question-begging.

5.4. Operational Coherence

Central to Chang's pragmatic realism is his notion of operational coherence which figures in his characterization of EA, SP, truth and reality. Operational coherence is introduced as:

1) A success term with probative value to judge and guide epistemic activities.
2) A more useful alternative than truth as correspondence in guiding scientific practice

Before we proceed, we need to get one thing out of the way: Chang thinks that the notion of truth as correspondence is empty but nothing in the above two claims hangs on that. In fact, what the two claims above say, particularly 2), is that coherence can do whatever proponents of correspondence think their notion does for them in guiding practice, and coherence can do it even better. So Chang is promising a practical added-value, over and above what correspondence claims to offer. Before trying to assess whether coherence does offer that added view, we should ask ourselves whether coherence succeeds in preserving the initial value that proponents of correspondence claim their notion possesses, that being the 'probative value'. I borrow 'probative value' from the legal context and transform it for my own purpose.

In law, the 'probative value' of a piece of evidence, sometimes called 'probative force', is meant to capture the power of this piece of evidence to support an alleged proposition. Unlike in philosophy where 'evidence' is often identified with beliefs, mental states, statements, etc. all of which have a propositional form – even evidence as relation also broadly falls under the same heading in virtue of being a relation between propositions (Kelly 2006), what counts as 'evidence' in law includes the propositional, e.g. testimony, as well as the non-propositional, e.g. a physical objects (Garner 2009, 835). So the 'probative

value' of a piece of evidence is in this sense *indifferent* as to the nature of what it is a power of, be that propositional or non-propositional. Let us call this *conceptual indifference*.

This is pertinent because Chang is clear that, with his action-based framework, he wants to distance himself from propositions, so we want a way to appraise his notion of 'operational coherence' in a non-question-begging fashion. Hence, appraisal using the usual notions of truth and justification will not do. We need something that would countenance, but is not committed to, either the propositional or the non-propositional, i.e. we need this *conceptual indifference* which, I believe, is what 'probative value' offers. At least, this is the aspect of the notion that I want to hold onto.

Alternatively, I do not want 'probative value' to be a power to support *propositions*, at least operational coherence is not meant to do that for Chang so we cannot use probative value as such. Hence, In the context of philosophy of science, I want, while maintaining its *conceptual indifference,* that probative value denote the power of whatever it is a power of – be that a proposition, a relation, physical object etc., to support that we are minimally 'getting things right' in the world. It might be objected that 'support' also won't do as it also has to do with evidence whereas coherence and correspondence are not evidence. Here I should note that I am using 'support' not as evidence but as something far less minimal and far less committal. It could even be said to be used as 'indicator' so long as we accept that indication may come in degrees. The difficulty in expressing the point, I believe, lies in my trying to provide a non-question-begging way to assess two different, very different, things in operational coherence and correspondence, both of which appeal to success to support that we are getting things right in the world.

Following this characterization, it should be clear that truth as correspondence, granting that the notion makes sense, wears its probative value on its sleeves in virtue of being a correspondence relation between proposition and mind-independent world. But we have the right to ask where does 'operational coherence' get its value from? This will be the focus of the remainder of this essay, beginning with Chang's characterization of it.

In characterizing operational coherence Chang writes:

> …an activity is operationally coherent if and only if there is a harmonious relationship among the operations that constitute the activity; the concrete realization of a coherent activity is successful, ceteris paribus; the latter condition serves as an indirect criterion for the judgement of coherence (2017a, 111).

Elsewhere he writes that

> It may be best to take 'harmony' (or 'harmonious') as a primitive in its meaning, *and verifiable in the end only through the achievement of the aim of the activity* (110, my emphasis).

From this and other explanations Chang gives we learn that operational coherence is:

a. a harmonious fitting-together of operations, where harmony is taken as primitive in meaning, and verifiable in the end only through the achievement of the aim of the activity.
b. concerned with *actions*, thereby going beyond mere logical consistency of propositions.

Concerning the question of criteria offered by Chang for judging coherence we get two answers:

c. It follows from a. that coherence itself is also verifiable in the end only through the achievement of the aim of the activity.
d. The concrete realization of a coherent EA will generally, ceteris paribus, lead to success.

For a start we can say that characterizing operational coherence in terms of harmony where harmony is taken as a primitive notion does not help in the right way in clarifying what coherence is supposed to mean. This perhaps can somewhat be remedied by providing many concrete examples of what are taken to be coherent and successful EAs and pointing out how different operations therein harmonise. Nonetheless, this still does not tell us where coherence gets its probative value from.

We get a hint at that when we look at the criteria suggested for judging coherence. What c. and d. capture is that there is a positive relation between coherence and success. So depending on the kind and strength of the relation, it may be that *success is what gives coherence its probative value*. In what follows I explore the relation that exists between coherence and success.

5.4.1. The Relation between Coherence and Success

I begin with the suggestion that coherence is a *sufficient* condition for success. That is if the EA is concretely realized we get success. Consider the activity of match-lighting, which is one of Chang's examples. This involves holding the matchbox firmly with one hand and holding the match firmly with the other, pulling the head of the matchstick across the rough strip on the box at an appropriate angle and at the right speed, then, finally, stopping the movement of that hand once the flame appears. As things stand, this EA once concretely realized leads to success, and, following Chang's claim that success is a good indicator of coherence, we say that the EA of match-lighting is coherent.

Now consider a very similar scenario to the one mentioned earlier where a gust of wind blows at the last instant and upsets the lighting of the match (2017a, 111). If we had performed the EA the same way we did before then we cannot, on pain of inconsistency, but say that the EA was coherent. Yet, in this example we had coherence but not success, so we cannot say that coherence is sufficient for success. But perhaps it is *necessary*?

Consider another scenario similar to the ones above, where the difference is that as soon as one holds the matchbox in one hand and the match in the other, lightning suddenly strikes the match causing it to light. Here, despite that we were not able to complete all the operations, success, i.e. getting the match lit, was achieved. Yet we cannot say that the EA was coherent because on almost any other occasion simply holding the match and the matchbox do not lead to success. Here, however, success was dependent on the lightening striking the match and not on the coherence of the activity itself. That means that coherence is not a necessary condition for success either.

Chang is well aware of situations similar to the ones just mentioned. Importantly, he accepts that coherence is neither necessary nor sufficient for success. Nonetheless, given his claim that the coherence of an activity ceteris paribus leads to the achievement of aims, he maintains that it is *a cause* of success. Yet, given that success can be appraised independently of coherence, and that coherence is dependent on success for its appraisal 'in the end' it seems that the notion of coherence is almost redundant. In fact, Chang comes close to admitting that when he says that coherence and success may mean the same thing but later rejects that option because we may have coherence while failing to have success as in the case of the wind above. All of that makes answering the question concerning the source of coherence's probative value all the more difficult.

5.4.2. Coherence as Guiding Practice

But perhaps we got off on the wrong foot when we sought the probative value of coherence from without the notion and its function in EAs. Recall that according to 1) in the introduction, coherence was supposed to accomplish what prima facie looks like two tasks with respect to getting things rights in the world.

i. Judge EAs.
ii. Guide EAs.

To illustrate the difference between i and ii, consider a case where we are assembling a high-tech device such as a smart phone. With respect to ii., we may be able use coherence to help adjust and assess the *process* of the assembly activity to get things right in that process if we are familiar enough with the ins and outs of the device including the roles of its component parts and how they fit together. Alternatively, with respect to i., we can wait *until after* the assimilation is done and the device is turned on to check if it works. So we have judgement simpliciter and guidance *for the process.*

Recall that I said this in aid of understanding what 'probative value' consists in: 'probative value denote[s] the power of whatever it is a power of – be that a proposition, a relation, physical object etc., to support that we are minimally 'getting things right' in the world. In this sense coherence can be said to have a probative value, which I said correspondence wears on its sleeves, when it is shown to help us judge EAs.

It is very likely that Chang, given his practice-based approach, would find the way I framed the question of assessing coherence in terms of probative value, which coherence is supposed to have in virtue of it helping us judge EAs and *additional* practical added-value which it virtue of it helping us guide EAs is misguided. He would probably add that this misconstrual is due to my failure to appreciate the real weight that practice has in his new framework.

The probative value of coherence, he would explain, is not restricted to it helping us judge EAs. Guiding EAs has probative value as well. Thus in response to our question Chang would say that *coherence acquires its probative value from its very ability to guide scientific practice*. If that is cogent then we can claim to have made progress in understanding Chang position which will eventually help us assess his claim that coherence is better than correspondence in practice. The question now becomes to what extent does coherence succeed in *guiding* practice?

As noted in c., it follows from Chang's claims that coherence is verifiable in the end *only* after the achievement of aims. Yet this limitation that Chang imposes on coherence actually defeats its aim in *guiding* practice as it blocks our ability to assess EAs in the process so it is advisable to drop it. This goes well with Chang's later remarks that coherence, unlike correspondence which can be understood as an all or nothing concept, comes in degrees (2017a, 107).

This means that we can have coherence as well as incoherence in the same activity – this option, Chang may contend, is not available for proponents of correspondence. As such, he would maintain that *while assessing activities we can pick up signs or symptoms of incoherence. This allows the notion to be useful in guiding practice.* Examples of symptoms of incoherence that Chang gives include false beliefs, mutually incompatible beliefs and lack of ability i.e. muscular ability, inappropriate materials (109). But how useful are these symptoms of incoherence in practice? Let us test that with an example.

5.4.3. Testing Coherence in Practice: Locating the Cellular Nucleus

Consider the activity of locating cellular nuclei using fluorescent microscopy. In order to do that we must first stain the cells with 4',6-diamidino-2-phenylindole (DAPI). DAPI binds to the adenine and thymine (AT) region of double-stranded DNA (dsDNA) which, when the cell is not dividing, is located in the nuclei. Upon excitation, DAPI emits a blue colour which can be seen under a fluorescent microscope. All of this is presumably factual information. Now let us consider scenarios where we have false beliefs about some of the information above and see how they may affect the coherence of our epistemic activity.

Let us begin with the case where someone does not know that DAPI binds only to AT but instead believes that it binds to all nucleotides (A, T, C, G) indiscriminately. If someone with such a belief does stain the cell with DAPI and looks under fluorescent microscope, she will

be able to very accurately locate the nuclei. Thus, this obviously *very relevant* yet false belief does not affect her nuclei-locating activity. Given that coherence comes in degrees and we are managing to achieve our aim, it doesn't seem unreasonable to say that in this case that the person is carrying out a coherent and successful activity. One may respond to this by saying that this false belief is not entirely false, indeed it could even be said to be 'approximately true' for the person has the approximate truth that DAPI binds to AT–along with partial falsehood that it binds to CG as well.

Now let us consider the case where someone believes that DAPI binds *only* to CG. This is a 'more serious' false belief because it lacks the 'approximately true' part which says that DAPI binds to AT. But as before, this false belief serious as it is, would still not affect her nuclei-locating activity which would also be 'coherent enough' and successful because at the end of the day she is achieving the aim of accurately locating the nucleus. But now some may grant that believing that DAPI binds *only* to CG is a relevant, serious, false belief which leads nonetheless to a coherent successful activity, but they may hold that there is still some truth to it. After all, when DAPI is binding to AT, it is binding to DNA and CG is DNA so in some stretched sense of approximate truth we can still make the argument that the falsity in the belief is really not *that* serious. But how much are we willing to stretch the notion of approximate truth before it becomes empty? This brings me to my third and last case.

Consider someone who does not know that DAPI binds to DNA *at all*. Instead she believes that it binds to the nuclear membrane. How would that affect her nuclei-locating activity? To answer that we need to first note that the typical eukaryotic cell (which is basically a cell that has a nucleus) undergoes a cell cycle that lasts around 24 hours. This is divided into interphase and mitosis, which last around 23 hours and one hour respectively. During the interphase the cell has a nucleus and the chromosomes are contained within it. Only during mitosis does the nucleus disintegrate only to reintegrate again after around an hour. Thus a cell spends approximately 95% of its time in interphase wherein the chromosomes are contained in the nucleus. This means that roughly, if someone believes that DAPI stains the nuclear membrane she will be able to carry out a coherent, successful nuclei-locating activity roughly 95% of the time. In this case, the person performing the activity would be holding false as well as contradictory beliefs and still be able to carry out a coherent and successful activity. Let me explain.

A person who holds that DAPI stains the nuclear membrane would be holding a number of false beliefs and two contradictory beliefs. These include the belief that 1- DAPI does not bind to AT, 2- DAPI binds to the nuclear membrane, 3-DAPI has a low membrane permeability (otherwise it would not bind to the nuclear membrane, it would go directly inside it). However given that in order for the person to believe that DAPI does enter the cell – in order to stain the nucleus, she needs to believe DAPI has high membrane permeability – for it to cross the cytoplasmic membrane. Yet, both the cytoplasmic membrane and the

nuclear membrane are roughly made of the same material which is a phospholipid bilayer. So the person would have to believe (whether she is aware or not) that DAPI has a high permeability and low permeability at once. Interestingly, that person would be holding all these false, some of which are contradictory, beliefs and still be able to carry out a successful nuclei-locating activity roughly 95% of the time. Such an activity with such a high success rate, given criteria supplied by Chang, I maintain should be considered operationally coherent.

Chang is likely to welcome this example and respond that it actually supports his account rather than challenges it. After all, the notion of operational coherence, he argues, comes in degrees. All of the activities above were more or less coherent evidenced by the fact that we are successfully achieving our aims of nuclei-location. I wish not to dispute that. But what I would like to do is to draw attention to two difficulties.

First, coherence in guiding practice is meant to have a probative value, which I previously clarified as the power to prove that we are getting things right. As such, having all these false and contradictory beliefs which, Chang tells us, are symptoms of incoherence should warn us that the EA is likely going to fail. Yet if we take the last case, which is the most drastic, we still get success roughly 95% of the time, meaning that at least in this particular activity these signs of incoherence are helpful in neither judging the EA nor guiding practice.

Second, given the way Chang characterizes the truth of a claim in terms of the latter's being pragmatically necessary for carrying out an EA, then a claim such as "DAPI binds to the nuclear membrane" should also count as true which is absurd given our current understanding. To this Chang may respond that such a claim is not *really* necessary for success. After all, what is necessary is in fact that "DAPI binds to AT". But such a response seems to me to be a two-way street. That is I see no reason why someone cannot claim that that DAPI binds to AT is not really necessary because it could be said to bind to the nuclear membrane.

Here Chang can make the following move and claim that although the success of the activity given the claim that DAPI binds to the nuclear membrane is roughly 95%, its success given the claim that DAPI binds to AT is 100% and we should favour the claim which gives the highest success rate. Granted, but then the idea of the highest success rate would need to feature in Chang's characterization of truth and as things stand it does not.

But even given this move, Chang is still not entirely off the hook. That is because an activity with the claim that DAPI binds to all nucleotides indiscriminately would have the same success rate as that featuring the claim that it binds to only AT. Hence, a similar argument to the one above can be run, however this time Chang does not have the turnover move to block it.

Chang could protest that the examples given above are far from typical and as such would not jeopardize his account but that would be missing the point. There is a reason why we are able to come up with such examples and dismiss them as being atypical. The reason is that we take the claim that DAPI binds to AT to be true, literally true and not pragmatically true relative to an EA. If the latter were the case, then we would have two 'relative true claims' which pull in opposite directions, neither of which can, given Chang's current criteria at least, claim to be what really is the case. Here, the idea of activity or mind-independence that correspondence claims to offer seems to be playing an important role in giving us this much needed 'fixed frame of reference' which we use to tell the typical from the atypical, without it, malign relativism seems to loom.

There is also a response which I saved for last. This could arguably overcome the challenge above and its ilk, and claim Chang victor. But, this, as will be clear, would amount to a pyrrhic victory. In line with Chang's counting his position a form of scientific realism, I have been, for the length of this chapter, trying to provide the most realistic reading possible of Chang's pragmatic realism. Later we will seriously consider to what extent we can consider Chang's pragmatic realism a form of realism. But for the time being, we will take for granted that it is a form of realism, that's why I take the following response to amount to a pyrrhic victory of Chang's pragmatic realism to the extent that it is a form of realism.

Given Chang's practice-based framework, his notion of pragmatic truth and reality are dependent on the concrete realization of a coherent and successful EA. This dependence can be understood in two ways. One way, which I have been so far assuming, is to say that coherent and successful EAs warrant the pragmatic truth of the related statement and reality of the entity mentioned in it so long as we are dealing with *that* and relevantly similar, system. So the claim remains pragmatically true even after the activity has been carried out so long as we are wary of the context in which we are making such a claim. On this reading, within the same SP and under relevantly similar conditions, a claim that we know to be false, given our current understanding, but which Chang's account allows to be true, causes us to seriously doubt the 'realism' of Chang's pragmatic realism.

However, the other way to understand the dependence of pragmatic truth and reality on activity is *indexical*. That is to understand the pragmatic truth of a statement and the reality of an entity strictly given *thi*s or *that* particular EA. In this sense the pragmatic truth of a statement and reality of an entity are *never* 'detached' from the *same* EA that warranted their truth and reality in the first place. So if we consider the objection above, for instance that DAPI binds to the nuclear membrane, that statement would count as pragmatically true *with respect to that particular EA* which was concretely realized by that particular agent in that particular place, at that particular time. On that reading none of what we took to be false beliefs above can be considered false, not even that DAPI binds to the nuclear membrane. Also accepting that a statement and its negation can both be true does not lead to a contradiction if each is used in a different EA. This response may salvage Chang's

position but it makes very difficult, perhaps outright impossible, that anyone would seriously consider this to be a realist view, and this I believe is enough to rule out this interpretation.

It is worth noting that there may indeed be a way to give Chang's indexical reading a more realistic twist. That is if we introduce a distinction between 'type EAs' and 'token EAs'. Claims, given this distinction, could then be indexed to type EAs but not to the token EAs thereby maintaining a sense of projectibility and possibly allowing for a more credible sense of realism – I will use the notion of EA type below. Yet this option, I believe, is not open for Chang, at least given the way that he characterizes EAs, for there he insists that EAs are *concrete* and not abstract, and types are supposed to be abstract.

As such, given that none of possible responses considered seem satisfactory, what transpires is that operational coherence, for Chang,

- Turns out to be a modest concept that is neither necessary nor sufficient for success and criteria for assessing it are mere symptoms that it may fail.
- It is verifiable in the end, according to Chang, only once success is achieved (a condition which we suggested should be dropped if coherence is to guide practice) and
- Even then success is not a sure sign of coherence and
- In practice we may pick up signs of incoherence which may act as red flags although, as shown in the example above, they do not necessarily upset success.
- Given its current construal coherence allows for incompatible claims to be both true even within the same SP.

This is already an unhappy conclusion for the prospect of operational coherence being able to successfully guide practice. But, keeping in mind that Chang claims that coherence is supposed to be able to do that better than correspondence, it appears that proponents of truth as correspondence can make similar claims when it comes to the role of their notion guiding practice. That is, given the low bar that Chang has set, proponents of truth as correspondence can claim that

- like coherence, correspondence is also (more firmly) verifiable only once success is achieved.
- There are signs of lack of correspondence such as false beliefs and contradictory beliefs, just like there are signs of incoherence.
- These signs although not guaranteeing failure at success can act as red flags.

More importantly truth as correspondence does not allow for incompatible claims. So we would not have embarrassing conclusions such as that DAPI binds to the nuclear membrane and that DAPI binds to AT are both true.

At bottom, it seems that Chang's notion of coherence has two difficulties:

1. It is not clear that it is able to *reliably* guide practice (the examples given above, admittedly atypical, cannot be shown to be so given Chang's criteria).
2. It does not seem to fare better than correspondence at guiding practice.

Although both claims constitute difficulties for Chang's pragmatic realism, the first is more threatening. In what follows I try to strengthen Chang account of coherence so as to make it more fitting for realism.

5.4.4. Strengthening Operational Coherence

Given the difficulties above, it is perhaps unsurprising that Chang is not entirely satisfied with his characterization of the relation between coherence and success. At one point he came close to conceding that success and coherence may amount to the same thing before he finally settled on coherence being a cause of success, in the sense explained above, which we have already shown is too weak. We can begin to improve on Chang's account by removing the ceteris paribus clause from his characterization. After all, it seems that the only role it is playing here is to protect Chang's characterization. What's more, I claim that in practice we do not use the ceteris paribus clause, instead we need to fill in local details – lots of local details, to bring about success.

Let us reconsider the example I gave earlier about the lightning lighting the match. It may not be entirely satisfactory for some, after all there is a reason why the saying goes "lightning never strikes the same place twice". But perhaps this strange example is a sign that we should not aim for unqualified success, but for something more specific which is less susceptible to such counterexamples. Chang was already thinking along those lines when he maintained that he does not want his notion of coherence to be "tied to accidental successes and failures determined by case-by-case variations of fringe circumstances" (2017a, 114).

That being said, I am not entirely sure what Chang means by 'accidental success'. The two meanings that I can find, however, organically belong to scientific practice and should not, as such, be discounted. These are:

1. Accidental as unexpected.
2. Accidental as unintentional.

Let us first note that at any moment in time we are epistemically constrained by what we know. So, to achieve a particular aim, we may have more than one set of different, yet coherent, operations which can ceteris paribus lead to success. For instance, in the case of the match-lighting activity, we may pull the match against the match-strip but we may also push it. These operations are different and so are the sets that include them. It could be that we never expected the pushing to be conducive of success but find out that it actually is. What this is meant to show is that we may have different sets of operations all of which, however, lead to the success of the same activity-*type*. This is a case, and accordingly a

sense, of accidental success which we would not want to disqualify. I now turn to the second sense which is accidental as unintentional.

Prima facie, it seems like 'unintentional success' is self-contradictory. After all if success is achieving one's aim and unintentionality is lack of aim, then it seems that there is no way that we can have both. However, many successes in science have been unintentional. A scientist performs an EA to achieve a particular aim; she notices that her experiment yielded some striking results which are not conducive to her *initial* aim but which are nonetheless worth further exploring. As a result, she changes her aim to suit the new findings and ends up replicating the same activity so as to reach this new aim. This is prevalent throughout the history of science, from the discovery of penicillin to the production of the first plastic, etc., so discounting unintentional success in this sense is also undesirable.

I believe that Chang would not want to disqualify either sense of accidental above, although these are the only meanings of accidental that I could find. But perhaps it is a slip, perhaps Chang did not really mean accidental in the ordinary sense. The citation above shows that he is, I believe, less concerned about accidents and more about 'fringe circumstances'. This sounds right, but then what we should do I believe is to aim not at eliminating 'accidental success' but at qualifying the kind of success that we are willing to tie coherence to.

My suggestion is that we should tie coherence to *systematic* success. But this only solves half of the problem. That is because introducing systematicity can make coherence a necessary and sufficient condition instead of it being merely a cause of success on Chang's account. This, thereby, redeems its probative value and its ability to guide practice. However, it still leaves the possibility of having incompatible claims such as DAPI binding to AT and DAPI binds to all DNA indiscriminately being both true, which I said is something we do not want to have on a realist account. On closer inspection we will see that this also can be avoided.

Going back to Chang's characterization of coherence, it seems that he was keen on making room for, prima facie, incompatibility *inter SP*, i.e. between incompatible SPs such a phlogiston-based SP vs an oxygen-based SP. This, however, has led him to overlook incompatibility *intra an SP*, i.e. between EAs within the same SP. If we discount what I called the indexical reading of Chang's account, we can then say that, clearly, Chang does not want claims such as that DAPI binds to all DNA indiscriminately and that DAPI binds only to AT to be both true. But there is nothing in his criteria that preclude that.

A way to avoid this, I believe, is to introduce SP into the characterization of operational coherence. By that I mean that the coherence of the EA which Chang so focuses on should be subject to the coherence of the entire SP. So, for instance, in the case where we want to locate the nucleus it seems that both claims above can do. Yet, given that they are incompatible we want to suspend judgment and check how other EAs within the same SP go. It will turn out that in trying to stain a CG rich region with DAPI, the activity fails, yet

when trying to stain an AT region the activity succeeds. This leads us to conclude that DAPI does not in fact bind indiscriminately to all DNA. Hence to ensure the overall coherence of the entire SP, we want to preserve compatible and mutually supportive claims and eliminate incompatible ones. As such we can characterize coherence as follows:

> Given a system of practice *SP*, a certain aim *X*, and an epistemic activity EA(X) (as and where it is actually carried out), *EA*(*X*) is operationally coherent iff within *SP*, *EA(X)* is systematically conducive to *X*.

As for what we mean by systematic success, I offer the following characterization:

> Given *SP*, *EA*(*X*) is systematically conducive to *X* iff the conditions *C* under which *EA*(*X*) is concretely realized are mutually supportive enough, given *EA*, to allow for the reliable achievement of *X*.

What I am aiming at here is to relax Chang's overemphasis on actions and to allow a place for propositions in the form of conditions. Chang, we should keep in mind, in challenging the proposition-based framework, he is not simply making way for 'action' in epistemology, which I take be sensible. Instead, he is proposing a radical shift by denying propositions the fundamental conceptual role in epistemology, which I take to be extreme – I say more on that in the postscript where I provide some remarks on the epistemology of practice. For now, I content myself with saying that strengthening coherence requires linking it to systematic success, which in turn requires that we countenance conditions that must be satisfied. These would have to be mutually supportive in that one or some of these conditions on its/their own would not be enough for achievement of the aim. They both need to coexist in a certain relevant fashion. For instance, for a protein to accomplish a particular task in the cell, not only should it be composed of a particular number of amino acids combined in a particular order, it should also exist in particular pH to ensure its proper folding, neither of these on its own would do. Many of these I take to be true in the correspondence sense – for reasons that go beyond this discussion, but Chang can still accept them as true in his own pragmatic sense.

Some of these conditions seem to exist together naturally while others hold only under regimented conditions. Consider again Chang's match-lighting example. He tells us about all the operations that must be coherently performed to bring about the lighting of the match. But what he fails to mention are the conditions that must also hold for our activities to lead to success. Such conditions include that the air be sufficiently oxygenated or dephlogisticated (bearing in mind Chang's pluralism), that the matches not be wet, that the air not be very damp, that there be friction etc. The conditions I have just mentioned are almost always there, they exist naturally and they allow for a systematically successful match-lighting activity. But how can we determine what conditions must be mutually present given a particular EA to arrive at systematic success? The answer to this question cannot be determined a priori, it must be determined in practice.

Generally, once systematic success is achieved, we can start to control for conditions to assess which conditions have a role to play, given EA, in the bringing about of the regular achievement of aims. Did my wearing my lucky green shirt play a role in the match-lighting activity? I change my shirt and find out. It is important to emphasize that assessment is bound to be contextual. Lightning may not be a condition that exists regularly enough to engender a systematic success in match-lighting activity in most places on earth, but in places like Lake Maracaibo in Venezuela, where Catatumbo lightning is a regular phenomenon, it may well be.

It is noteworthy that a similar dissatisfaction with Chang's notion of operational coherence has been expressed by Eleonora Montuschi (2020) in her discussion of objectivity given Chang's practice based-framework. Montuschi recognizes the merits of Chang's new framework but claims, as I do, that his notion of operational coherence has certain limitations. Her claim is that the coherence of operations leading to the achievement of the inherent purpose of an EA may fail to achieve the external function of that EA. This, she holds, is a drawback of Chang's account. As a remedy she suggests that we should complement Chang's operational coherence with two other criteria. One is *relevance* and the other is *reliability*. She maintains that

> Relevance gets established on the basis of a number of assumptions that account for why we rely on a particular activity. I might know (learn) from other activities (other than match lighting, e.g. scratching a dirty pan with a scourer) that if I'm too soft on a rough surface there will be no sufficient friction to get any effect going. So, I need to import knowledge and expertise from other activities where that very operation is used for other purposes and make it relevant to the new aim (11).

I agree that relevance and reliability have a role to play in Chang's story, and that making them more explicit strengthens Chang's account. This should be obvious given that my own construal of operational coherence has reliability built into it. Nonetheless, I believe that Montuschi's objection misses the mark. Her claim that operational coherence is deficient because an EA may be coherent yet fail to achieve its external function rests on a misunderstanding of how coherence works. The immediate aim according to which Chang holds the coherence of an EA is to be assessed is the inherent purpose and not the external function of the EA. True, we should choose those operations that are relevant for achieving the external function, hence my agreement with Montuschi on the importance of *relevance*, but to criticize Chang's operational coherence on the grounds that it fails to achieve what it did not purport to do in the first place, I believe, is an unfortunate mistake.

I believe that including SP in the characterization of coherence and tying the latter to systematic success where systematicity is characterized in terms of mutually supporting conditions given an EA has at least three advantages over Chang's. First, it allows for a stronger relation between coherence and success in a way that makes the former necessary

and sufficient for the latter thereby salvaging its probative value. This is especially pertinent as Chang wants coherence to guide practice. Second, it blocks incompatible claims belonging to different EA but to the same SP_2 from being both true. Third, it helps to relax Chang's overemphasis on action at the expense of propositions by giving each its due.

However, this, if successful, solves only the first problem, i.e. it gives a more robust construal of operational coherence, one which can be used to guide practice, but it does not support Chang's claim that coherence is better than correspondence in practice because, as we said earlier, Chang's appeal to *symptoms* such as false and incompatible beliefs as ways to guide practice is equally available to proponents of correspondence.

Importantly, proponents of correspondence who are sympathetic to Chang's pleas for a practice-based philosophy of science may agree with him on the central role of operational coherence in guiding practice almost down to the last detail without feeling the need to relinquish correspondence. They would allow that Chang makes it seem that coherence and correspondence are incompatible – they surely are on his account. But a minor conceptual shift would reveal that, pace Chang, not only can these be compatible, but more importantly they can be complementary. I consider how this can be done in the last section where I provide general remarks on what a realist epistemology of practice can be. But before I get to that I want to take a step back and consider two general questions, which I have postponed until after I presented the details of Chang's position. These are 1- broadly in what sense is Chang's position pragmatist, i.e. how does his position relate to those of the classical pragmatists?, and 2- in what sense, if at all, is it realist?

5.5. Chang amongst the Pragmatist

To answer the first question, I begin by considering Chang's own interpretation of pragmatism. He notes that

> The most fundamental point about pragmatism, as I take it, is that knowledge is created and used by intelligent beings who engage in actions in order to live better in the material and social world (2019, 11).

This remark broadly captures the zeitgeist of pragmatism as a tradition that emphasizes the central role of agency in the world (Legg & Hookay, 2008). The latter is highlighted in Chang's framework by making the aims of agents defining features of EAs and SPs and replacing the proposition-based framework with an action-based alternative. More specifically, pragmatism has been understood as a theory of meaning, a theory of truth, and as an overall method of learning. Chang subscribes to all three.

Pragmatism as a theory of meaning was first introduced by C. S. Pierce (1878) with his 'pragmatic maxim'. It has since received many formulations. A famous one due to William James is:

> to attain perfect clearness in our thoughts of an object, then, we need only consider what conceivable effects of a practical kind the object may involve— what sensations we are to expect from it, and what reactions we must prepare. Our conception of these effects, whether immediate or remote, is then for us the whole of our conception of the object, so far as that conception has positive significance at all (1907, 46-47).

This semantic interpretation of pragmatism was accepted by James as a "method for settling metaphysical disputes" (45). Chang also accepts this interpretation of pragmatism as broadly inviting us to get clear on the content of our concepts by identifying the practical consequences of their applications (2019, 11). This, for him, makes semantic pragmatism in a sense continuous with the logical positivists' verificationism and Percy Bridgman's operationalism, the latter of which Chang is at pains to rehabilitate (see Chang 2017c). But Chang is not content with restricting himself to the semantic interpretation, for that, he holds, following Philip Kitcher (2012), amounts to 'domesticating pragmatism'. Instead, he wants pragmatism to be "a philosophy that helps us think better about how to do things, not just about what our words mean" (Chang 2019, 12). This brings us to the second understanding of pragmatism as a theory of truth.

I already said that Chang accepts a pragmatic theory of truth according to which a statement is taken to be true if belief in it is pragmatically necessary for carrying out a coherent and successful EA. Such an account of pragmatic truth could be said to either coincide with Pierce's account of truth as 'end of enquiry' or to significantly diverge from it. The ambiguity here lies in 'end' which can be understood either as the 'aim' or the 'final state' of enquiry (Haack, 1976).

On the former reading, Pierce's account and Chang's do seem to coincide. Suppose we understand Chang's EA to coincide with Peirce's 'inquiry', where the latter, for Peirce, is understood as a "process that takes us from a state of doubt to a state of stable belief" (Legg & Hookay, 2008). Then achieving the aim of the activity would be reaching the end of enquiry, and on both accounts whatever beliefs required to reach our end are taken to be pragmatically true in that sense. But taking the end as the 'final state' of enquiry, which captures the more popular reading of Pierce, does not. This is especially the case when we note that, later in his life, Pierce took truth as the final stage of enquiry to be a 'regulative ideal' and nothing that we can confidently claim to be converging on.

This reading of Peirce on truth is inconsistent with what Chang wants of truth. For one of the most important reasons why Chang rejects the correspondence notion of truth is that he thinks we can never know whether we have arrived at it (2018, 31). Chang wants truth that we can know when (or if) we arrive at it, one that is useful in the here and now of scientific practice (33), and not a regulative ideal that may not even exist.

Accordingly, Chang endorses James's view that 'the true is the expedient' (1907, 222). But he refuses to interpret 'expedient' as the merely 'useful' or 'convenient'. Instead, Chang interprets James's 'expedient' as whatever allows for successful experience when confronted by reality (2019, 13). In this sense James's 'expediency' is quite similar to Chang's 'pragmatic necessity' which is at the heart of both his theory of truth and reality. This brings me to the last sense of pragmatism that Chang endorses which is pragmatism as a method of learning.

Chang's interpretation of James on 'truth' which makes experience in the empiricist sense *the source* of truth by holding that the true is whatever is required for successful experience in the face of reality, allows him to construe pragmatism as a form of 'deep empiricism' which says that experience is "the only ultimate source of learning" (13). For Chang, the next step for pragmatism *qua* deep empiricism is to develop a learning method that is fit for purpose. The seeds of such a project, Chang holds, are to be found in John Dewey's (1938) *Logic- The Theory of Inquiry*, which tells us that the content of our learning along with our method of learning arises from successful habits of thinking, which we develop through the same process of enquiry. Chang considers his own pragmatic realism a development of the pragmatic method (2019, 15).

5.6. Realism, Perspectivism, and Relativism

I noted at the outset the challenge that faces any attempt to assess whether and to what extent a self-proclaimed realist position is indeed realist. Ideally we would have a crisp definition of 'scientific realism' with certain necessary and sufficient conditions. But as anyone who is remotely familiar with the scientific realism debate knows, the situation is far from neat and tidy. In fact, attempts to understand what scientific realism amounts to have resulted in a definitional morass, leading a key player in the debate to maintain that

> It is perhaps only a slight exaggeration to say that scientific realism is characterized differently by every author who discusses it (Chakravartty 2011).

That being said, Chakravartty does end up providing what seems to be a minimalist criterion that every self-proclaimed realist position should meet, namely that, qua realism, it should adopt a 'positive epistemic attitude' towards science. On that account, Chang's pragmatic realism is a form of scientific realism. But here we should keep in mind that, given Chang's radical break with the traditional proposition-based framework and his development of his action-based alternative, what adopting a 'positive epistemic attitude towards science' means for Chang is significantly different than what was originally intended by traditional realists. Hence, fully committing to the claim that Chang is a realist in the sense intended by Chakravartty requires a more careful consideration.

Traditionally, the positive epistemic attitude towards science has been cashed out in terms of Stathis Psillos's (1999) *metaphysical*, *semantic* and *epistemic* theses, which roughly say

that there is a mind-independent world, that our scientific descriptions of this world, both the observable and unobservable domains, are truth-conditioned, and that these descriptions are approximately true more often than not. Almost all later qualifications of traditional realism accept some form of Psillos's three theses.

Now if we compare Chang on realism to the more traditional account we realize that he only agrees with the traditional realists on the metaphysical thesis but disagrees with them on the rest. That is despite agreeing that there is a mind-independent world, Chang, I said, takes the mind-independent world to be akin to Kant's noumena about which "we can and should say nothing". This denies Chang a place amongst the traditional realists but it reserves him a place in the venerable tradition of neo-Kantianism, a recent variant of which, namely 'perspectival realism' is currently thriving in philosophy of science (See Massimi & McCoy 2020).

Chang takes to heart the kernel of perspectivism, namely the 'situatedness of our scientific knowledge', which says that scientific knowledge is both historically situated, i.e. influenced by the practice of the historical period, and culturally situated, i.e. influenced by the prevailing cultural tradition (Massimi 2017, 164). Moreover, he considers his pragmatic realism to be a form of perspectivism, albeit a 'deep' one, where a 'perspective' for him, in this context means a 'conceptual framework'. Chang distinguishes his deep perspectivism from shallower or milder forms. This he does while warning against domesticating perspectivism in the same way he warns against domesticating pragmatism.

What unites mild or shallow forms of perspectivism, for Chang, is that they all assume that the world already comes pre-parsed into objects and properties, which bars having incommensurable perspectival knowledge. Conversely, deep perspectivism, which is the result and not the starting point of Chang's pragmatic realism, denies the pre-parsing of the world, thereby denying the existence of perspective-transcendent ontological states of affairs, and accepting a plurality of perspectives which need not be connected, let alone, reduced to one another – although it does not reject that possibility outright either and considers it a question open for empirical investigation (2019, 22).

Some may complain that Chang's pragmatic realism is really disguised relativism. That is, with its deep perspectivism and metaphysical pluralism it cannot really be a form of realism. I think that Chang's response to this charge would probably be that his view is indeed relativist, but also that relativism is not incompatible with realism. So he is a realist and a relativist. To see how that is possible we need to look at what relativism is or can be.

Much recent work has been done to clarify what relativism means in general (Kusch 2019) and in the context of philosophy of science (Kusch 2021). What such works tell us is that relativism is a genus term under which many species fall. Kusch, following Susan Haack (2000), provides a general scheme to broach the question of relativism. This scheme is of

the form ‘x relative to y’. Kusch notes that depending on what ‘x’ and ‘y’ stand for we may end up with different forms of relativism, such as:

- Ontological relativism where ‘x’ stands for objects, properties.
- Alethic relativism where ‘x’ stands for truth(s).
- Semantic relativism where ‘x’ stands for classifications, concepts, or meanings
- Epistemic relativism where ‘x’ strands for epistemic justification.

And ‘y’ can range from individuals to cultures, to scientific paradigm, etc. (2021, 2).

Consider semantic relativism when philosophers disagree as to whether whales are fish or mammals (Dupre 1999). This kind of relativism seems benign with respect to realism. Or take the recent attempts by traditional realists to fortify their success-to-truth inference (Vickers 2019). This falls under epistemic relativism, but we also would not say that this form of relativism is incompatible with realism. If anything it seeks to support it. I could go on, but the point I am trying to make should be clear by now: there is nothing intrinsic about relativism that makes all species of it incompatible with realism.

That being said, Chang’s pragmatic realism perhaps includes all four kinds of relativism. His deep perspectivism makes him a semantic relativist, his denying that the world is pre-parsed makes him an ontological relativist, his emphasis that the truth of a claim is always given within a particular EA makes him an alethic relativist, and his claim that EAs take place within a SP_2 which determine the methodological rules makes him an epistemic relativist. With all these forms of relativism one may be tempted to dismiss the claim that Chang’s pragmatic realism is indeed realist but that would also be too quick.

What speaks in favour of Chang’s realist case despite the many relativists threads running through it is his emphasis on practice and reality. After all, Chang’s position is a practice-based one that aims to maximize our learning from reality. What is the tribunal for Chang is not individual taste, not culture and not scientific paradigms. It is successful scientific practice when confronted with reality which often refuses to cooperate and, in the words of James, forces us to correct our formulas. This, I believe, makes Chang pragmatic realism deserving of the realist label.

Many traditional realists will find this unsatisfactory. They will scoff at a realist position that not only fails to tell us anything about reality but also thinks it is impossible to do so. This for me is the Achilles heel of Chang’s pragmatic realism, not his pluralism, not his relativism, but the failure of a purported realist position to tell us anything about reality. The middle ground then is not to deny that Chang’s position is realist but to highlight that it is *not realist enough*. Nonetheless, I believe that traditional scientific realists ought to benefit from Chang’s position and overall practice-based framework by distilling some of the key insights of his that they can use to strengthen their own positions. In the postscript I provide some reflections on Chang’s epistemology of practice and point out how this can be done.

5.7. The Relevance of Chang to the Scientific Realism Debate

It is commonplace in contemporary philosophy of science that scientific practice is central to answering philosophical questions about science (Soler et al. 2014; Chao et al. 2016). Accordingly, the last 40 years or so has seen a number of practice-based approaches to philosophy of science such as the new experimentalists which include Ian Hacking (1983), Peter Galison (1997), Allan Franklin (2012) and Nancy Cartwright, whose views I discussed in Chapter 4, as well as the establishment of the Society for Philosophy of Science in Practice (SPSP), with notable members including Rachel A. Ankeny, Joseph Rouse, Mieke Boon, and Hasok Chang.

Pertinent to the scientific realism debate is that nowadays both realists and antirealists, in their argumentative strategies, make the claim that their position provides a better account of scientific practice (Kukla 1994). Yet a glance at the realism debate literature reveals that in most cases scientific practice receives no more than lip service. This is especially the case given that most versions of NMA on offer today are inferences from some form of predictive success to truth of theories, without enough, if any, attention being paid to the scientific practice responsible for this success. That is, with the exceptions of few outliers (notably Waters (2019), almost none of the realists provide an epistemology of scientific practice, one that links products to process.

What Chang's pragmatic realism does is to provide exactly that: an epistemology of scientific practice which does not simply defer to 'theory' but aims to take into account other non-propositional aspects that are essential for bringing about scientific success. Pertinent to that is Chang's notion of operational coherence which is a means to assess scientific practice that goes beyond mere logical consistency of propositions and takes into account the activities that are taking place throughout and which Chang rightly argues are constitutive of scientific success. So when it comes to answering the question of how Chang's pragmatic realism helps to advance the realism debate, particularly when concerned with unobservable entities I say the following.

In section 4.7. I have noted that Cartwright construed as a modelling-based realist is an invitation for scientific realists to be wary of the scope of their realistic commitments. There I said that if the unit of philosophical analysis are system-specific local models then our realistic commitments should also be local. Chang's pragmatic realism, particularly with his epistemology of practice, adds another constraint to our realistic commitments. If Cartwright says that they need to be local, Chang adds that they also depend on a particular set of practices. Accordingly, when stating our realistic commitments we should always flag them with a rider which says that "so long as those particular set of practices responsible for bringing about the success"-- then unobservable entity X exists.

5.8. Conclusion

Pragmatic Realism is Chang's attempt to reorient the realism debate away from truth and towards practice. It replaces the common proposition-based framework by an action-based alternative. It does away with truth as correspondence for 'operational coherence' which Chang introduces as a success term with probative value to guide practice. It ultimately accepts a pragmatic theory of truth and reality, whereby a claim is considered true and an entity real if taking them as such is pragmatically necessary to carry out a coherent and successful epistemic activity. I argued that coherence construed as neither necessary nor sufficient is too weak and fails to guide practice. I offered a stronger notion of coherence which makes it necessary and sufficient for *systematic* success. This salvages the probative value of coherence but leaves the question of its superiority over correspondence in practice open. In the final analysis, I take Chang pragmatic realism to be worthy of realist label despite the many relativist threads going through it, since it aims to maximize our learning from reality, even if it falls short of telling us anything about it.

Postscript to Chapter 5: Remarks on Epistemology of Practice

At bottom, Chang's new framework is suggested as a response to the overemphasis on theory to the detriment of practice (similar worries have been voiced by other practice-oriented philosophers of science, see Waters 2016 and Boon 2017 and references therein). Yet, when motivating his framework, Chang carries over the criticism he levels against theories in order to discount propositions as well. This move, however, seems neither explicable nor warranted. Just because theories fail to be exhausted by sets of propositions, it does not follow that propositions need to be eliminated in a practice-centred approach. Here we are not concerned with Chang's criticism of correspondence as an account of truth, which is arguably the reason for his dissatisfaction with propositions. For the debate here is methodological. Chang's criticism is that a theory-based or even a proposition-based framework fails to do justice to scientific practice.

Dissatisfaction with theory is not really a peculiar feature of Chang's framework as in the last 30 years philosophy of science has seen theories lose hegemony, particularly to experiments (Hacking 1983, Radder, 2003, Arabatzis 2013), scientific models (Morgan et al. 1999; Morrison 2007, 2015; Cartwright 1999, 2019, Cartwright & Suarez 2008), and more recently measurement (Tal 2013, 2015; Reiss 2010, 2016) and even narrative (Morgan & Wise 2017). This is taking place alongside pleas for 'theory eliminativism' (Vickers 2014; French 2020). What is peculiar is the way Chang transforms propositions. On Chang's account, in taking the backseat to actions propositions lose everything that makes them interesting from a realist standpoint, namely that they tell us *about* reality. This, I pointed out, is what I take to be the Achilles heel of Chang's pragmatic realism and, it is, arguably, this that makes traditional realists consider Chang's not to be a serious form of realism. But this I believe can be remedied. With few amendments, Chang's powerful framework I think can be given an equally powerful realist interpretation. In what follows I give a few pointers as to how this I think can be done.

An on-going, but perhaps implicit theme that runs through Chang's discussion is that simply explaining the success of science in terms of truth, as realists do, fails to appreciate all the complexity and interconnectedness of different practices that go into the bringing about of this success. Not to mention that these successes, just like the practices that bring them about, are local and so should be evaluated as such (See Cartwright 2020). This in a sense echoes Alexander Bird's (2021) overall dissatisfaction with second-order arguments in the realism literature, such as NMA and PMI, for not bringing to the table new evidence for or against the truth of scientific claims. Notwithstanding Bird's claim, the point is that truth is not doing much evidential work in explaining the success of science *in practice*. It is in light of these considerations that Chang introduces his notion of operational coherence.

Notice, however, that coherence here is playing a *justificatory* role. It is helping us to justify a set of practices. But truth and justification are two different things which Chang fails to properly distinguish throughout his discussions, especially when trying to show why

coherence is better than correspondence. If my claim that coherence is playing a justificatory role is cogent – this may not be Chang's favourite reading but his own work does lend itself to such an interpretation especially when it describes coherence as the *source* of truth (2017a), then his comparison between correspondence truth and operational coherence is flawed from the get-go. That is because it is a comparison between a theory of truth in the form of correspondence with what may be taken as theory of justification in the form of operational coherence. But there is nothing problematic about a position that combines these two. In fact, Laurence Bonjour, for example, has defended a similar combination in his *Structure of Empirical Knowledge* (1985).

Similarly, recalling my liberal construal of epistemic support in my discussion of probative value as being one that involves minimally 'getting things right' in the world and includes the propositional as well as the non-propositional, I think we can interpret Chang's 'operational coherence' as a theory of epistemic justification within the domain of science – and if Chang wishes even outside science, without, however, relinquishing correspondence as an account of truth.

On that account, when an EA is deemed operationally coherent this lends support to the sum of both the propositional and non-propositional components that were relevant for the coherence of that EA. These include theories, laws, local claims, as well as concepts, experiments, instruments, measures (this is not exhaustive). Many of these are not candidates for truth and falsity so they are not directly relevant to the realism question, at least as traditionally construed.

As I am sympathetic to Cartwright (2019) with her suggestion that laws and theories do not express propositions and that they cannot be rendered to express propositions without either being false or losing universality, what I take to be relevant for realism are local models and the local claims that can be constructed from these models. When an EA is coherent as I have characterized coherence, then we have good enough reasons to accept that the local model is representative of the target system and that, pace Chang, local claims that we construct from these models are true in the correspondence sense so long as we do not take those claims beyond those and relevantly similar domains.

Arguably, Chang might think that reinterpretation defeats his original aim, which is to banish correspondence from philosophical discussions. But I believe that for realists who see virtue in Chang's practice-based framework yet want to maintain that their claims are *about* reality in a more robust sense of reality than Chang's 'little r' reality, a combination of operational coherence as the theory of justification and a correspondence theory of truth seems ideal. Working out the changes that must be made to accommodate this conceptual shift and how the framework will eventually end up looking is a task for another day, but for now these minor points will suffice.

Conclusion

Having gone over Putnam's Common Sense Realism, Cartwright's Modelling-based Realism, and Chang's Pragmatic Realism, it is helpful to provide a brief summary that links everything I have said in the previous chapters to the main aim of the dissertation before I am able to take stock.

In the introduction, I said that this dissertation has two aims, a direct aim which is to provide a critical exposition of what I take to be three unconventional forms of scientific realism that promise to advance the scientific realism debate and an indirect aim which is to provide groundwork for a new realist position that integrates what I take to be important in each of these positions.

I contrasted these three positions with conventional forms of realism which I claimed to be deficient at at least three levels. First, I said that conventional forms of scientific realism generally take science to be at least in tension with common sense and at most as having shown common sense to be false. I showed in my discussion of the later Putnam why such a claim is unacceptable. Second, I have said that conventional realism have for the most part taken theories as the loci of philosophical analysis and I explained in chapter 4 and 5 why this is a mistake. Third, I said that conventional scientific realist, indeed all current players in the debate pay lip service to scientific practice but very few provide an epistemology of scientific practice that links practice to success.

In what follows I shall explain how I think each of the three unconventional realist positions can help advance the debate. I provided a characterization of the realism debate, which in line with recent developments adds questions to the traditional ones that go beyond those of truth of theories/claims and the reality of entities. These include questions that make more prominent the role of scientific practice in the debate such as Arabatzis new challenge to make sense of *successful scientific practice* that was centred on entities that are thought to no longer exist, or the plea for an epistemology of practice that links scientific processes (e.g. experimentations) to scientific products (e.g. theories, models claims). Importantly, I said that another more metaphysical question has gained prominence, namely, "what is the *world* like given our successful theoretical practices?", which, I said, draws on the two traditional questions of the truth of scientific theories and the reality of scientific entities but does not reduce to them. It does not reduce to them because first, it ignores what it might mean to 'take a positive scientific attitude' (recall Chakravartty's general description of scientific realism from the introduction) to scientific practices and second, it suppose a view about what the world is like, to wit that it is a world of things displaying features whose behaviours can be subsumed in the systematic way we expect from theory. Having said that, I summarize the three unconventional realims' contribution to the debate.

The later-Putnam's contribution to the debate, as explained in sections 1.5. and 3.6. is in making the debate over unobservables *possible* by mitigating the semantic sceptical worry

brought about by MTA through the adoption of direct realism in perception. Also, his states of affairs ontology which supports the phenomenon of conceptual relativity promises to help to deflate PMI, and the success-to-truth inference by showing how representations positing incompatible objects can be true in their intended domains. As such, what were thought to be counterexamples (e.g. phlogiston) to success-to-truth inferences no longer are, for the realist can maintain that the inferred entities are real, not in the sense that the objects posited really exist but that the representations positing these objects at the level of the 'surface grammar' succeeds in representing the 'object-less states of affairs' accurately in an object/feature language. Lastly, Putnam's conceptual pluralism and levels of form contribute to the broader metaphysical task of providing the most general description of reality given our successful scientific theoretical practices by providing a way to maintain that both the common sense picture and the scientific picture are equally correct, with each 'designating' a level of form.

Cartwright's contribution to the debate lies in drawing the realists' attention to the importance of choosing the right unit of analysis and how that impacts the unit of commitment. I explained in section 4.7. that given that MbR chooses local models then our commitments should be equally local, whether these are commitments are to entities, structure or what have you. In this sense, Cartwright adds another layer of localism to the debate over and above the two types discussed in that same section.

Chang's contribution to the debate as explained in section 5.7. lies in providing an epistemology of scientific practice that links scientific process to products. As explained, if Cartwright's MbR tells us that our commitments should be local then what Chang reminds us is that they are always dependent on a particular set of practices. Hence combining Cartwright's and Chang's insights, realists ought not to make unqualified realistic commitments, instead their commitments should always be domain and practice specific.

I am now in a position to take stock. The aim here is not to favour one view over the other but to provide groundwork for a new realist position that combines the best of each into a synthetic whole. These are:

- Putnam's direct realism and conceptual pluralism
- Cartwright's model particularism
- Chang's pluralism and his epistemology of practice

One of the key lessons derived from the later Putnam is that any viable scientific realist position must start with a direct view of perception. Although the middle Putnam was committed to the veracity of science, he rightly noticed during his later period, as I show in section 3.3.2., that a realism that is only internal to science, i.e. one that does not even attempt, let alone manage, to hook onto the world is closer to scientific anti-realism than it is to realism. Scientific realism, at the very least, assents that science teaches about the mind-independent world. In order to do that it must have access to that world. Our access

to the world is attained through perception. The latter can either be direct or indirect. But indirect accounts of perception are susceptible to MTA. As explained in section 3.3.1., MTA makes reference radically indeterminate thereby making realism a non-starter. Lest we accept some form of supernatural access to the world, salvaging realism, Putnam insists, as I note in section 1.2., requires adopting a direct theory of perception.

Direct realism gives us access to the mind-independent world thereby making scientific realism *possible*. But what is needed is an argument to make scientific realism *compelling*. This is where NMA comes in. I have noted in section 3.2. as I was discussing Putnam's convergence realism that NMA may give reason for the reference and truth thesis but not the convergence thesis. But the convergence thesis is necessary for only a particular brand of realism, i.e. *convergent* realism. But, as I argue in section 3.5., we can be scientific realists without be convergent realists.

There is much insight to be gained from Chang and Cartwright on arguments for realism. It is my contention that the claim that science teaches us truth about the world in the correspondence sense, albeit a liberal sense of correspondence, can be rendered more robust given these philosophers' remarks on success. To see that let us begin by noting that successful science-to-truth arguments whether in the form of NMA or other arguments, requires that we be clear about at least two things:

1. What success amounts to and
2. What the unit of such success is.

It is tempting to think that what is meant by unit of success is the unit *responsible for* success. But, as my discussion unfolds, it will become clear that 'unit of success' also has other meanings and I want to make use of these as well. But for the moment I would like to use 'unit of success' loosely and will sharpen my use as I proceed.

Putnam, despite paying lip service to scientific practice, agrees with almost all other traditional realists that the units of scientific success are 'theories'. Here is where Cartwright and Chang's criticisms become pertinent. Both practice-based philosophers, coming at the topic from different angles, think that this is a mistake.

On the one hand, Cartwright, we have seen, argues convincingly that the success of science, in the form of predictions and technologies, neither is nor can be derived from well-confirmed theories. I noted that Cartwright holds that a close attention to scientific practice leads us to accept the "models as mediators" thesis according to which models by mediating between theory and reality play an essential role in bringing about scientific success. So the unit of success for Cartwright are not theories but system-specific models.

Chang, on the other hand, we saw, tries to distance himself from theories and even propositions altogether and takes the units of success to be epistemic activities and systems of practice. His contention is that emphasis on theories leads to a distorted analysis of

science as it neglects all the practice that go into bringing about scientific successes. Both philosophers, in their own way, emphasize the dependence of scientific success on local facts and practices.

Yet, despite Cartwright and Chang's agreeing that theories as units of success are not fit for purpose, they seems to be in tension when it comes to deciding what the better alternative is. Is it the system-specific models as holds Cartwright? Or epistemic activities broadly construed as holds Chang? Here I would like to note that this tension is at least superficial and at most remediable. But in order to show that I must first pay closer attention to what is meant by the unit of scientific success.

In section 4.3., I have noted that it is commonplace for selective realists nowadays to make the distinction between 'idle' and 'working posits'. That is, starting from the analysis of a purported successful scientific theory we are able to make distinctions between those parts of the theory that are *really* 'doing', thereby warranting our realistic commitments and those that are not, hence 'idle' (Vickers 2017).

What the structure of the selective realist strategy shows is that we can break down what I called the 'unit of scientific success' into 1) units of analysis and 2) units of commitments. By unit of analysis I mean what we look at when we want to assess the success of science. Most realists nowadays, including Putnam and the selective realists, take the units of *analysis* to be theories. The disagreement between selective realists is on what the unit of *commitment*, i.e. what is 'doing work' or what is *responsible for* the success, should be, whether it is 'entity', 'mathematical structure', etc.

One may wonder why we should even care about the unit of analysis if after all what matters in the end for realism is the unit of commitment. The answer is that the choice of unit of analysis affects the appropriate unit of commitment (e.g. choosing universal theories implies that our commitments should be universal in scope). Having explained the roles that 'unit of analysis' and 'unit of commitment' play I can now apply those to Cartwright and Chang's positions.

Accordingly, we may be able to say that for Cartwright, who holds that theories are but one component in the building of local models, these local models are themselves the unit of analysis, and, when successful, i.e. when we are able to make successful prediction and produce reliable technologies, particularly when that success depends on the model, then the local model is the unit of commitment. From there we can construct local true claims about that and relevantly similar systems. Simply put, the unit of analysis and unit of commitment, for Cartwright's MbR, generally coincide in local models.

As for Chang, it is clear that his unit of analysis are epistemic activities. His units of commitment are entities that satisfy his coherence theory of reality and claims that satisfy his pragmatic theory of truth. But there is no place for models on his account since these are

meant to represent the mind-independent world which, for Chang, we cannot even begin to talk about, let alone successfully represent. Nonetheless, I believe there is a way to bring these positions together, albeit with some modifications.

What I am looking to develop is a position that that allows me to wed Chang's epistemology of scientific practice with Cartwright's strong, albeit local, realist conclusions. This I believe can be done by taking Chang's epistemic activities as the *unit of analysis* and coupling that with local system-specific models as the *unit of commitment*. This allows me to bring together the insights of both philosophers.

Chang's account of epistemology of practice with epistemic activities and system of practice as units of analysis brings to the foreground the, often ignored, role of the full range of what we *do* in science beyond just what we *claim* in bringing about scientific success, such as measurement, modelling, instrument building, etc. And while Cartwright agrees with that she does not offer such an epistemology so her account is lacking at that level. Chang's account also helps to bring down to earth the realism debate from abstract theories far removed from actual scientific success, to the more local matters of facts and practices that are responsible for such success, something Cartwright has long stressed. But once everything is in place and the success is had, what are we warranted in committing to? Here's where Chang's account breaks down, at least for strong-minded realists to whom I belong, and Cartwright's insights comes into effect.

The feature of Chang's account that so many realists, including me, find unsatisfactory is his claim that successful scientific practice teach us not *about* reality but only *from* it. This especially pertinent given Putnam's argument described in section 3.3.2., that any form of realism minimally construed must purport to tell us something about the mind-independent reality. Chang's pragmatic realism shies away from this. This emphasis on mind-independence is neither merely terminological nor peculiar to Putnam and in fact is shared by almost all traditional realists with whom I side on that point. The condition that for scientific realism to qualify as *realism* must tell us something about the mind-independent world may understandably be taken to be too stringent as it not only discounts Chang's pragmatic realism but possibly many others. I maintain that it is fair nevertheless. Realism is a robust notion and should not be watered-down.

By contrast, Cartwright's account provides the scientific realist who is sympathetic to Chang's practice-based epistemology but not to his pragmatism something that satisfies her. Cartwright offers a locus for what we learn *from* reality in local system-specific models that represent min-independent facts or states of affairs. These play a central role in securing scientific success. So we are thus, by the usual success-to-truth inference, warranted in counting them true. It could thus be said that it is where Chang's account breaks down that Cartwright's account takes over. This captures the almost natural synergy between these two accounts. Chang supplies Cartwright with an epistemology of practice

that she lacks and Cartwright supplies the strong realistic conclusions that Chang's account eschews.

I said that arguments from success to truth, which form the backbone of any form of realism, must be clear on what success amounts to and on what the unit of success is. The foregoing discussion drew lessons from Chang and Cartwright about the units of success, now I draw lessons about what success is or can be.

On Chang's account, success is neither novel prediction nor any other form of success usually suggested by the selective realists. It is rather the achievement of (epistemic) aims of particular agents through the concrete realization of epistemic activities. Having forgone truth in the strict realist sense and replaced it with operational coherence and pragmatic truth, Chang offers an inference from success to operational coherence. But he reassures us that once an activity is deemed operationally coherent a number of ensuing statements can be deemed pragmatically true. It should be clear that this, however, will not impress the strong-minded realist.

But disagreeing with Chang on that point far from makes his framework futile. The recent status of the realism debate and its fixation on prediction, novel prediction, risky novel prediction, very risky novel prediction as indicators of truth should warn us that the debate has gone sour. Vickers's forthcoming work on future-proof science (*forthcoming*) serves to emphasize that, against realists' common practice, we should not place too much weight on predictive success as indicative of truth. Vickers suggests that a more promising candidate is 'scientific consensus'. Irrespective of what we may think about the epistemic status of scientific consensus, Vickers's suggestions bring to the fore an important point that, despite two decades or more of work in social epistemology, is still too often ignored in the contemporary scientific realism debates, namely that science is a community-based and not an individual-based practice. Chang's notion of operational coherence construed as a property of epistemic activities *given* a system of practice offers a more promising alternative.

If we, pace Chang, maintain a strict realist notion of truth that is dear to the traditional realist while accepting Chang's notion of operational coherence as a notion of epistemic justification of the whole battery of scientific processes and products – as I pointed out in the postscript to Chapter 5, we are able to provide a more sophisticated argument to truth than the one usually offered by traditional realists, one that is grounded in community-based scientific practice. For the very notion of systems of practice that Chang offers already includes a community dimension. Systems of practice with their rules, standards, and overall research agendas are developed in a scientific community and by a scientific community. But it also includes a material practice dimension, i.e. activities need to be concretely realized.

Hence, what we arrive at is a new construal of the success-to-truth argument. For the purpose of warranting the truth of claims, success, given this new account, requires operational coherence, and an activity is deemed operationally coherent only given a system of practice. This makes the success that is relevant for the success-to-truth arguments very hard-won. For it is no longer acceptable on this account to go from novel prediction to truth. The latter may be impressive in its own right but unless it can be shown to have taken place through the performance of a coherent epistemic activity, recalling my account of coherence from section 5.4.4. this means through the performance of an activity that arrive at systematic success in accordance with the standards set by a particular system of practice, then it does not warrant an inference to truth. This while keeping in mind that those very standards naturally evolve as we make new discoveries and develop new practices, so what was once deemed unacceptable may later become quite welcome.

More precisely, if we apply my distinction between unit of analysis and unit of commitment and accept Cartwright's insights, as I think we should, then we would say that given this epistemology of practice, the new argument for scientific realism would be one where we infer not from predictive success and so forth to truth, but from the operational coherence of an epistemic activity to the faithfulness of the representation in the corresponding system-specific model to the targeted states of affairs. From the latter we construct local true claims in the strict realist sense, this is while keeping in mind that these truths as well as the ontology prescribed by the model are restricted to these and relevantly similar domains. Taking a page out of Cartwright's book, we can say that *when* an activity is deemed operationally coherent, then the entities represented by the corresponding local model are real.

We now arrive at the final theme common amongst all three philosophers, namely pluralism. Each philosopher espouses a different variant of pluralism, but these need not clash. Cartwright with her rejection of universal laws, and her stress that roughly different domains can submit to different principles defends what we may call domain-specific pluralism. Putnam, as is the case with most traditional realists, is happy with universal laws, but he is keen to maintain the reality of our common sense world, hence his defence of what he called 'conceptual', which in truth is ontological, pluralism. Also, recall from section 3.4.1., that noticing that some physical representations with prima facie incompatible ontologies may be successful, Putnam further came up with 'conceptual relativity'. But Putnam never took prima facie incompatible ontologies, i.e. objects of different and incompatible conceptual schemes, to be reflecting *real* incompatibility, for the descriptions furnished by these conflicting schemes are equivalent, he claimed, and *fundamentally* there *really* are no objects in such domains. Chang explicitly defends metaphysical pluralism according to which the ontology featured in incompatible theories, for example oxygen and phlogiston, can both be real given different practices. I suggest that all these pluralistic positions are mutually compatible.

Although Chang does not explicitly discuss laws as he is most concerned with activities, his epistemology of practice according to which a statements may be considered pragmatically true *in a given circumstance* shows that he shares Cartwright's dissatisfaction with universal laws and theories. It could even be said that Chang's framework, in a way already assimilates Cartwright pluralism if we take Chang's 'circumstances' as being none other than Cartwright's 'domains'.

And although what allows Chang to make a case for metaphysical pluralism is that the reality of 'theoretical entities' as well as truth of the corresponding statements are *pragmatic* – otherwise we would have flagrant inconsistencies, Putnam's 'ontology without objects' allows us to have a more realistic take on Chang's metaphysical pluralism without getting inconsistency. That is because if what is ontologically fundamental in a certain domain are not objects but states of affairs which are represented using different object-based schemes, then surface inconsistency there does not do violence to what is fundamentally there.

Thus, a realist position that I hope to construct growing out of this thesis, integrating insights from Cartwright, Chang and Putnam, is one that:

- Accepts direct realism in perception and the reality of common sense ontology.
- Accepts Chang's epistemology of practice, a strict realist notion of truth while transforming operational coherence into an account of epistemic justification.
- Accepts the distinction between unit of analysis and unit of commitment with the former being epistemic activity and the latter, local system-specific models.
- Rejects universal laws and theories, and accepts domain-specific – practice-based-pluralism.
- Accepts that in certain domains, particularly physics, we have objectivity without objects, thereby allowing for conceptual relativity and ontological pluralism.
- Accepts conceptual pluralism of which conceptual relativity is one species.

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